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CHEMICAL PATHOLOGY OF CHOLERA.

REMARKS ON "DR. THOMSON'S ANALYSIS OF THE BLOOD OF INDIVIDUALS AFFECTED WITH MALIGNANT CHOLERA." *

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It has afforded me much satisfaction to perceive, that Dr. Thomson of Glasgow has devoted his attention to the investigation of the chemical pathology of the malignant cholera. As the results he obtained are of considerable importance in deciding the question, what are the *universal facts* which chemistry has ascertained in the pathology and etiology of this disease, I deem myself not unprofitably employed in laying before the readers of this journal an outline of Dr. Thomson's investigations, accompanied by some passing remarks which they will occasionally elicit. My object throughout is to contrast Dr. Thomson's results with those described in my own report on this subject, to show in what points they differ, and in what they correspond,—where difference exists to examine how far the processes employed by either experimentalist may have influenced the result; finally to derive from these inquiries whatever conclusions they legitimately afford, as to the *essential* and *universal* chemical facts ascertained concerning this epidemic.

In the commencement I should briefly state, that the results of my own experiments indicated, *first*, a material diminution of water in the blood of the cholera patient, the specific gravity of the serum ranging from 41 to 54; *secondly*, a notable decrease in the quantity of soluble salts, amounting, as far as regards the serum, to a mean loss of one-third of these substances; *thirdly*, that the solid constituents of the crassamentum, including its salts, retained their normal *proportions*, wanting merely the addition of water to restore it to the standard state; and, lastly, that the dejections were alkaline and albuminous, and contained the water and soluble salts in which the serum of the blood was deficient.

In the first of these results, Dr. Thomson's analysis corresponds very closely with mine. Of five specimens of serum, the specific gravities were as follow:—

Standard of health, 1.0280.

1. 1.0446 pure yellow serum.
2. 1.0443 very slightly tinged red.
3. 1.0520 very red.
4. 1.0550 very red.
5. 1.0570 a very deep red.

In my experiments conducted in Newcastle, and published in my report, the specific gravities stood thus:—

1. 1.0410 pure yellow serum.
2. 1.0450 ditto.

In the experiments conducted in London, of four specimens the specific gravities were:—

1. 1.0427
2. 1.0540
3. 1.0470
4. 1.0513

To these I may add, the results of the analysis of Rose of Berlin, which afford, as the mean of his experiments, 1.0447.

In an analysis, in other respects extremely faulty and imperfect, published by Hermann, of Moscow, the specific gravity of the serum was also greatly increased beyond the natural standard. I conceive, then, that we are fully entitled to consider the universality of this fact in the chemical pathology of cholera, as completely and satisfactorily proved.

The second division of Dr. Thomson's paper treats of the proportions between the serum and crassamentum; a question very difficult to solve *in a mechanical point of*

* Philosophical Magazine and Annals, May, 1832, Art. XLIX, Chemical Analysis of the blood of Cholera Patients, by Thomas Thomson, M.D., Professor of Chemistry in the University of Glasgow.

view, owing to the obstinacy with which the crassamentum retains the serum in its interstices. Indeed it would be wrong under any circumstances, to pronounce with apparent accuracy on these proportions from mechanical computation alone. We may, however, with some justice for the present, assume the standard to be 57 serum to 43 crassamentum.

In the only case in which I had leisure to make this kind of estimation of the proportions in cholera blood, they were 43 serum and 57 crassamentum, being precisely the reverse of the normal standard.

In Dr. Thomson's five specimens, the apparent increase in the solid ingredients was still more remarkable, viz.—

	Serum.	Crassamentum.	
1.	32.34	.. 67.66	serum pure yellow.
2.	32.—	.. 68.—	ditto slightly red.
3.	38.45	.. 62.58	ditto very red.
4.	35.66	.. 64.34	ditto very red.
5.	27.59	.. 72.41	ditto very deep red.

The mode of analysis here requires particular attention.

In my analyses the proportions were thus ascertained. As much serum as possible was first mechanically separated from the crassamentum. A portion of the serum was then evaporated to dryness to ascertain the amount of its solid contents. *As the water of the crassamentum exists therein in the state of serum*, a portion of crassamentum was then evaporated to dryness, and the loss of water noted. The data afforded by the analysis of the serum, thus gave me, by a ready calculation, the precise amount of serum retained by the crassamentum.

Dr. Thomson, on the other hand, simply "poured off as much of the serum as possible, the crassamentum was then poured on a cotton cloth, and left for twenty-four hours, till all the serum that would had dropped through the cloth, the weight of the remaining crassamentum was then determined." This proceeding is obviously not quite so exact as the former, and tends to exaggerate materially the proportion of the crassamentum in the specimens he examined.

Dr. Thomson's third section refers to the composition of cholera serum. He assumes Marcet's and Berzelius's analysis for his standard, viz.—

	Berzelius.	Marcet.
Water	90.5	90.00
Albumen ..	8.0	8.68
Salts	1.5	1.32

In the experiments on cholera serum, Dr. Thomson's results were as follows, and I have to request particular attention to the circumstance, that *he calculates the albumen and salts together.*

	Water.	Albumen and Salts.	
Standard	90.5	....	10.
No. 1.	83.95	....	16.05 serum pure yellow.
2.	83.96	....	16.04 slightly red.
3.	81.17	....	18.83 very red.
4.	81.1	....	18.9 very red.
5.	80.82	....	19.18 very deep red.

In my experiments I assumed Lecanu's analysis for my standard, and estimated the albumen and salts *separately*, a proceeding which I believe to be preferable, for reasons which I will subsequently explain. I also estimated by the 1000, not by the 100. In order, however, to contrast my results more intelligibly with Dr. Thomson's, I may thus state the issue of the two analyses made in Newcastle, taking the albumen and salts together in the mode practised by Dr. Thomson.

	Water.	Albumen and Salts.
Standard	90.60	9.0
No. 1.	85.40	15.70
2.	86.68	13.92

It is thus seen that my experiments here agree with Dr. Thomson's in again denoting a great deficiency of water, and *a corresponding increase of the albumen in the serum* compared with the normal standard. Dr. Thomson moreover, in his analysis, makes no allowance for the fatty matter which Lecanu and Denis have recently proved to exist in healthy blood, and which occured in both my analyses of cholera serum, and which are associated with the albumen in Dr. Thomson's analyses. The difference between us in the amount of albumen is thus shown to be more apparent than real. It is also to be remembered, that in no two cases can the ingredients of cholera blood be numerically the same, so completely disorganised is the condition of that fluid. Thus, in my own analysis of various specimens of serum, there existed these differences between Nos. 1 and 3.

	No. 1.	No. 3.
Water	8.54	8.66
Albumen	1.33	1.24
Salts	5.80	2.92
Urea .....40.—		—
Fatty matter	1.43	1.28

This fact is an instructive one, as it proves mathematically that we are not to look for strict numerical correspondence in the results of various inquirers into the *chemical* pathology of this malady. Accordance in the broad results can alone be expected.

The next section of Dr. Thomson's paper, on the salts of cholera serum, is one to which I wish particularly to solicit the attention of the reader, inasmuch as some writers have theorised very liberally on the presence, absence, or varied proportion, of these ingredients in cholera blood, and have even based some remarkable thera-

peutic propositions on the second of these assumptions. I shall therefore examine this section in some detail. If, in doing so, I should have occasion to offer objections to the indications which Dr. Thomson's experiments apparently afford, I trust that distinguished and respected individual will believe me, when I state that my object in this inquiry is solely the eliciting of truth. I have no theory or opinion to support, and had I one I would cheerfully abandon it the moment it was rationally shown to be incorrect.

In order to render the subject as clear as possible to the general reader, I deem it necessary to premise these observations by stating, that our knowledge of the composition of the blood has been greatly increased within the last year, by the instructive and numerous analyses of M. Lecanu, of Paris, detailed in his prize essay, read before the Academie de Medécine. To my mind his experiments are altogether faultless, and his results are consequently pre-eminently entitled to be received as our standard and guide in all inquiries concerning this fluid. According to this eminent chemist, normal serum consists of water, a fatty matter, *an organic substance soluble in alcohol and water, a compound of albumen and soda soluble in water*, various saline matters partly soluble in water, and partly insoluble in that fluid.

Of these ingredients, the organic matter, soluble in alcohol and water, and the albumenate of soda constitute 3.79 per 1000 parts.

Premising these remarks, I may now with more advantage approach Dr. Thomson's experiments on the quantity of salts in cholera serum.

"To determine the quantity and nature of the salts contained in cholera serum, 394.36 grains of serum were exposed to the temperature of 159° in order to produce coagulation. The coagulum thus obtained was dried in a heat of about 200° till it ceased to lose weight. The whole was now reduced to 48.85 grains."

On this step of the process, which is intended to expel all the water of the serum, I feel it necessary to offer a brief observation. There is scarcely any process which requires more patience than the complete drying of albumen. When coagulated at first it retains much water and uncoagulated albumen in its interstices, and then from its imperfect conducting power much of the fluid albumen contained in these interstices is solidified, owing to loss of water, without undergoing coagulation. A firm pellicle or crust is also formed on the upper surface of the albuminous cake, which tends to retain much of the hygrometric moisture. For these reasons it becomes absolutely necessary to remove the cake from the vessel, when apparently dry, to triturate it carefully in a mortar into minute powder, to heat it again in a temperature of 212°, and continue this manipulation with much assiduity until the powder is almost impalpable. If this precaution be neglected the accuracy of the subsequent steps in the analysis is a little interfered with. The next sentence in Dr. Thomson's analysis illustrates the truth of this observation.

"This dry matter, in the state of a coarse powder, was digested for two days in a Florence flask on the sand-bath. The whole was then thrown on a filter, and the liquid which passed through was evaporated to dryness. This water had dissolved a notable quantity of albumen, which was separated during the evaporation in a hard transparent crust. I endeavoured to separate it as completely as possible by dissolving the salt three times successively in water, and evaporating the solution to dryness. By every successive evaporation some albumen was separated; but after the third evaporation I could obtain no more albumen by this process. The saline matter remaining weighed 2.39 grains: it was grey, acted powerfully as an alkali, and by the test of chloride of platinum appeared to contain both potash and soda."

"The salt contained no sensible quantity of sulphuric acid, nor of lime, nor of phosphoric acid: there existed in it 1.47 grain of common salt; the remaining 0.92 grain was alkali and a brown-coloured salt, which deliquesced, and which dissolved in alcohol: it doubtless also contained some albumen."

The digestion in water in the process thus described must have removed from the dried albumen, first, all the salts, such as the muriate and carbonate of soda soluble in water; *secondly, the organic matter soluble in alcohol and water; thirdly, the albumenate of soda*; fourthly, urea, a probable ingredient in cholera serum; and fifthly, *any uncoagulated albumen*. Now, for any of these organic matters, Dr. Thomson makes no allowance. In his tabular summary, he speaks of the 2.39 grains, as *salts*, without further comment or explanation; whereas it is manifest that a notable proportion of organic matter should have been deducted from this amount, for *the organic matter soluble in alcohol and water, and for the albumenate of soda*, independent of the albumen, much of which was separated during the process. In short, it is not at all improbable, that the 0.92 grain, or more than one-third of the entire, was the albumenate of soda, *and the organic matter soluble in alcohol and water*, the presence of which in serum has been

pointed out by Lecanu and Denis, and confirmed by numerous experiments of my own.

Dr. Thomson then proceeds:

"The albumen thus freed as completely as possible from salt by digestion, was dried and exposed to a red heat for half an hour in an open platinum crucible; it left a grey ash, weighing 0.77 grain: this ash was partly soluble in water. The portion dissolved was common salt and an alkali, weighing 0.53 grain. The remainder dissolved in nitric acid, and was precipitated in white flocks by ammonia and by caustic potash added in excess; it was, therefore, phosphate of lime: it weighed 0.24 grain."

From these experiments, Dr. Thomson thus states the composition of the cholera serum, No. 1.

Water	85.950
Albumen..	15.015
Salts	1.035 or 13-50 per 1000.

In my examination of serum, the salts obtained by solution in water, *were freed of the organic matter and the albumen of the albuminate of soda, by incineration*, before they were weighed, and accordingly my results were, in No. 1,

Water	85.40
Albumen	13.30
Salts perfectly free from organic matter	0.56

Had Dr. Thomson's mode of analysis been adopted, and *the organic matter soluble in water* been included in the estimation of the salts, my results in No. 2 would have stood thus:—

Water	85.40
Albumen	13.30
Salts	1.08

We thus find, that allowance being made for the organic matter associated with Dr. Thomson's "salts," his results and mine correspond in indicating a deficiency in the amount of the soluble salts in cholera serum. Indeed the analysis of the dejections evidently shows the presence of these salts in the rice water-fluid, and of course leads us to expect a priori, a diminution of their quantity in the blood.

Dr. Thomson's next statements on the quantity of these saline ingredients in cholera blood generally, are apt I fear to lead a superficial observer slightly astray. Dr. Thomson proceeds to furnish a table of the saline contents of the other four specimens of cholera serum, *calculated on the analysis in No. 1, and not derived from experiments on each specimen*. Now a single fact is sufficient to demonstrate the inapplicability of calculation to *this* branch of the investigation, and to prove the necessity of making each specimen the subject of separate analysis. In my examination of cholera serum *no two specimens contained the same quantity of salts*, although in all the average was below the normal standard. Even the nature of the salts occasionally differed. Thus, in 1000 parts—

	Healthy Standard. Lecanu.	Malignant Cholera. No. 1.	Malignant Cholera. No. 3.
Muriate of Soda and Potassa	6.08	4.00	2.17
Carbonate of Soda.. Phosphate of Soda.. Sulphate of Soda ..	2.10	No carbonate of soda, but 1.60 of phosphate and sulphate of soda, earthy salts and iron.	0.5
Earthy Salts and Iron	0.91	——	0.70

This fact is decisive proof that proportional calculation cannot be admitted into our reasoning on the quantity or kind of saline matters in cholera serum.

I regret much that Dr. Thomson has not given any account of the symptoms of the patients who furnished the blood on which he operated. It is to be expected that the amount of the saline ingredients of the serum which are soluble in water, must hold a marked relation to the amount of the purging experienced.

On the whole the consideration of this section of Dr. Thomson's paper, and the results of his single analysis relative to the salts, coincide with my experiments in showing, that as a universal fact the salts in cholera serum are diminished in quantity, but never completely absent.

The fourth section in Dr. Thomson's paper relates to the crassamentum. The amount of fibrin was found in the manner employed by Lecanu. The proportion of colouring matter was determined *by evaporating the water employed to wash the crassamentum to dryness in a temperature rather under* 212. The amount of saline matter was then examined by burning a portion of the colouring matter in a platinum crucible.

Before stating the results thus obtained, I find it requisite to notice the manner in which the colouring matter was examined, which tends materially to increase its apparent quantity. Besides the colouring

matter itself, the washing took away the large amount of albumen retained by the crassamentum in the form of serum, and amounting in the serum of specimen No. 1 to 15.015 per cent. The colouring matter we shall thus find was in this manner apparently increased nearly one third above its actual quantity in No. 1. Indeed Dr. Thomson does not describe the colouring matter in his tables as pure, but gives it as "mixed with albumen." Dr. Thomson analysed two specimens to determine the amount of colouring matter.

There is perhaps no other ingredient of the blood, concerning the standard proportion of which so much difference of opinion exists among eminent chemists, as with respect to the colouring matter. The processes again devised for its separation are quite as numerous, quite as opposed to each other, and many of them (like that employed by Dr. Thomson) exaggerating remarkably the amount supposed to be obtained. In my experiments, the plan pursued was one which mathematically admits of no fallacy, if the operator possesses ordinary manual dexterity. A certain quantity, say 1000 parts, of moist drained crassamentum is taken, the analysis of the serum having been previously performed. A part of the crassamentum is then evaporated to dryness, the residue triturated to powder, and subsequently heated, &c., so that all its moisture is accurately expelled. A simple calculation, founded on the previous analysis of the serum, then tells at once how much serum was retained in the moist clot, and this you deduct from the weight of the residuum.

Another portion is now washed to obtain the fibrin, the amount of which when dried is carefully ascertained.

A third portion is incinerated, and the amount of saline matter noted.

Deducting then from the 1000 parts of moist crassamentum, the water, the albumen, &c., of the serum it retained, the fibrin, fatty matter, and the salts, the remainder shows precisely and infallibly the quantity of colouring matter. This is the simple, beautiful, and unerring method devised by MM. Prevost and Dumas, followed by Lecanu, and adopted in my own experiments on cholera blood.

Another process, and one of much precision, is that practised by Denis, and pursued in the immense number of analyses described in his brilliant work.* It is founded on the fact, that albumen mixed with colouring matter, if much diluted, does not coagulate by heat, while the colouring matter, diluted with fifty parts of water, coagulates at 150 Fahrenheit.

The object of these processes is *to free the colouring matter from albumen.* That the results thus obtained are infinitely better entitled to be taken as our standard than those derived from the process followed by Dr. Thomson, *which retains the albumen, extractive, &c.*, amounts to a self-evident proposition.

In ascertaining the standard again, it appears from Lecanu and Denis's experiments, that the sex, age, temperament, and circumstances, of the individuals, must not be lost sight of. In some of Denis's experiments, for example, *the pure colouring matter amounted to* 18 *per cent.*† In others it was as low as 6 and 7. The *mean* of all Lecanu's experiments *gave* 13 *per cent* of *pure* colouring matter *perfectly free from albumen.*‡

We are now prepared to examine with more precision the indications afforded by Dr. Thomson's analyses of two specimens of cholera blood.

	No. 1.	No. 4.
Albumen	4.856	6.305
Fibrin	0.378	1.340
Colouring matter, *with albumen*	27.450	23.160
Salts	1.195	1.255
Water	66.121	67.940

At first sight the excess of colouring matter here appears considerable, but two circumstances tend to render this excess much more apparent than real. The first is the large quantity of albumen mixed with the colouring matter, owing to the deceptive process employed. A simple calculation suffices to show the precise amount of this source of error in the analyses before us.

The crassamentum in No. 1 (Dr. Thomson) contained of water 57.60 per cent. Now the water of the crassamentum exists therein in the state of serum.‡ The solid contents of that serum being soluble in water, are consequently included by Dr. Thomson in the sum of the colouring matter, because Dr. Thomson estimates the colouring matter by washing the *moist* crassamentum with water, and *evaporating the entire washings to dryness.* Now the previous analysis of the serum, belonging to that crassamentum (see *Phil. Magaz.*, page 353), showed, that exclusively of the salts, the serum contained of albumen 15.015 per cent. If then 83.950 parts of water are equivalent to 15.015 of albumen, 57.60

* Recherches Experimentales sur le Sang Humain Considéré a l'Etat Sain. Par Prosper Sylvain Denis. 8vo. pp. 358. Paris, 1830.

* See Denis Op. Cit., p. 142, 216, 222, 231.

† See Journal de Pharmacie, September, 1831, p. 392.

‡ Prevost, Dumas, and Lecanu. Journal de Pharmacie, p. 500.

parts of water are equivalent to 10.302, and a minute fraction, with which I need not encumber the calculation. We have, therefore, 10.302 to deduct from 40.57, the amount of colouring matter with albumen in Dr. Thomson's analysis of cholera crassamentum, which leaves the amount of pure colouring matter in 100 parts of blood, 20.480, because 40.570 of the impure colouring matter, containing 10.302 of albumen, 27.450 of impure colouring matter, must contain of albumen 6.970, which, deducted from 27.450, leaves of pure colouring matter 20.480 per cent.

Again, in his second analysis, No. 4, the water of the crassamentum was 60.66 per cent. Applying the same rule, we find that the serum of this specimen contained, according to Dr. Thomson's statement, p. 353, in every 100 parts—water 81.100, albumen 17.681. The 60.66 parts of water of the crassamentum are therefore equivalent to 13.224, and a minute fraction of albumen. The actual amount of pure colouring matter was therefore 14.651 per cent.

From these data it is manifest that the results of these two analyses, instead of denoting a great excess of colouring matter in the cholera blood, show that in No. 4 it had barely reached the normal standard as fixed by Lecanu, and fell short of the amount afforded in numerous analyses of healthy blood by M. Denis.

The standard of comparison, selected by Dr. Thomson, is unquestionably not deserving of confidence. I use these free terms, because it is not founded on Dr. Thomson's own experiments, but is the result of a single analysis, faultily performed by another individual, and long before Prevost and Dumas pointed out the important fact that the water of the crassamentum existed therein as serum, and that, consequently, a great deduction must be made from the quantity of colouring matter, *if estimated by the evaporation of the washings of the crassamentum.*

Again, another and most important circumstance to be taken into consideration, is the bulk or volume (contradistinguished from the *weight*) of the moist crassamentum, compared with the free or floating serum. If the crassamentum be large and loose, as it invariably is in cholera blood, or in any blood deficient in water or imperfectly coagulated, the crassamentum must retain a proportionately great quantity of serum, and the colouring matter (if analysed by Dr. Thomson's process) be associated with a great quantity of albumen. If, on the other hand, the blood coagulates firmly, as in health or in inflammatory affections, the crassamentum contains infinitely less serum, and the colouring matter will retain a proportionably small quantity of albumen, so that if analysed by the process followed by Dr. Thomson, the contrast between the standard and the cholera colouring matter, will be very striking indeed, and the apparent excess of the latter proportionably augmented.

Another observation will perhaps render this circumstance more readily understood. It will be admitted as a general proposition, that cholera blood is deficient in water. Now if a cholera patient and a rheumatic patient be bled to eight ounces each, the entire blood drawn in the former case will contain much less water than that in the second. But after coagulation, *the crassamentum of the cholera patient will contain more water* by many degrees than that of the firmly-contracted rheumatic clot. Now if an analysis of both be performed by Dr. Thomson's method, the cholera colouring matter may apparently amount to five times that of the rheumatic crassamentum, and yet be really in no excess.

It follows as an inevitable conclusion from these facts, that the sources of fallacy are strictly in proportion to the bulk of the crassamentum, so that by the process followed by Dr. Thomson, a pretty correct estimation of the amount of colouring matter may be made in the well-contracted crassamentum. It is, therefore, no proof of the accuracy of the process, if it affords the normal proportions in the analysis of a standard or healthy specimen of blood. It is in the morbid state, and especially in cholera, that the fallacy must most materially prevail.

To apply these remarks to the elucidation of the pathology of cholera, we find, that as far as these two analyses of Dr. Thomson bear on the question, the *pure* colouring matter is not necessarily increased beyond the normal standard, so that a healthy crassamentum of equal size and weight might contain precisely the same proportion of colouring matter with that of a cholera crassamentum. If, however, the serum be added to each in equal measure, then a given sum of the cholera *blood*, say 100 parts, would contain rather more colouring matter than the normal blood, *in proportion to the deficiency of water of its serum.* This increase, however, would not be very material; for example, the lowest amount of water in cholera blood, is in Dr. Thomson's analysis 67 per cent. The lowest in the *standard* analysis of Denis and Dumas 78, and of Lecanu 77, so that the difference of eleven per cent., supposing the standard amount of colouring matter 13, could not occasion more than an increase to 14.68, or 1.68 per cent.

The preceding train of reasoning is borne out by my own analyses, in which, in two

specimens examined in Newcastle and four in London, there were in each 100 parts :—

Pure colouring matter, No. 1	16. 25
2	15. 6
3	16. 43
4	14. 7
5	11. 28
6	13. 6

These results were obtained by an accurate repetition of Lecanu's process, by which the weight of the fibrin, salts, fatty matter, water, and albumen, are first ascertained apart, and then the remainder gave the colouring matter in a pure state. In several other specimens which I examined by another process, namely, by the washing of the clot and precipitation of the albumen from the coloured fluid by the acetate of lead, the quantity varied from 13 to 18 per cent. I do not, however, rely so confidently on this process as on the other, which, if performed with common dexterity, is not susceptible of fallacy.

My experiments, therefore, coincide with Dr. Thomson's, regarding the crassamentum also, when the deductions are made from his numbers in the manner I have shown to be essential. Even did our results differ, the circumstance would still be instructive, as my experiments would then show, that the supposed increase in the quantity of colouring matter was not connected with the *essential* pathological phenomena of the disease, in other words, that it was not *a universal fact*, and, consequently, that it could not be considered as one of the essential events of the disease.

The next part of Dr. Thomson's paper which requires attention, is that in which he alludes to the quantity of fibrin in the cholera blood, and of which he speaks in these terms :—

"The deficiency of fibrin in cholera blood is very remarkable, and struck me very forcibly. The fibrin in the cholera blood No. 1, is only about one-tenth of that in healthy blood; the quantity in the blood No. 4, is nearly four times as great; but it is still only about the third of the quantity which exists in healthy blood."

The quantity of fibrin in 100 parts of blood, according to the vague standard in Dr. Thomson's paper, is rated at 4. 45 per cent. In his cholera analysis in No. 1, the fibrin was 0. 378; in No. 4 it was 1. 348. Had Dr. Thomson consulted Lecanu and Denis, an inspection of their results would have spared him the trouble of speculating on the cause of the deficiency, and of inquiring whether "the increased quantity of colouring matter may be accounted for by any alteration in the fibrin." In point of fact, Dr. Thomson's specimens of cholera blood afforded very nearly the standard quantity of fibrin, as the following table will sufficiently prove. I take 20 examples from Lecanu, and as many from Denis :—

Quantity of Fibrin in Healthy Blood.

In one thousand parts. Lecanu.		In one hundred parts. Denis.	
1	1. 360	1	0. 25
2	1. 690	2	0. 25
3	1. 800	3	0. 27
4	1. 870	4	0. 40
5	2. 140	5	0. 30
6	2. 200	6	0. 25
7	2. 210	7	0. 27
8	2. 410	8	0. 25
9	2. 140	9	0. 27
10	2. 578	10	0. 27
11	2. 720	11	0. 25
12	3. 130	12	0. 28
13	3. 240	13	0. 21
14	3. 565	14	0. 31
15	3. 730	15	0. 27
16	3. 904	16	0. 35
17	4 250	17	0. 35
18	4. 630	18	0. 22
19	5. 000	19	0. 23
20	5. 030	20	0. 23

Compare these with Dr. Thomson's analysis of cholera blood :—

No. 1.	No. 4.
Fibrin 0. 378	1. 345 per cent.

The cause of the difference is simple. The standard of comparison selected by Dr. Thomson, gives 44. 5 per 1000, as the proportion of fibrin! an error dependent on the imperfect mode of drying resorted to by the experimentalist. Chevreul has satisfactorily shown that fibrin, when apparently dry, still retains *four-fifths* of its weight of water. Accordingly, in Dr. Thomson's own analysis, where I presume the fibrin was much more perfectly dried, normal results were very closely approached.*

* There still remains a topic in Dr. Thomson's paper on which I would minutely enter, had not my remarks already extended beyond the limits suited to a weekly journal. I allude to the quantity of albumen in cholera *blood* generally, which, in Dr. Thomson's tables, is far below the natural standard. Now, in the *serum* of the same blood, Dr. Thomson has shown that the albumen was double the quantity of the normal state, being 15 per cent in two specimens, and 17 per cent in three others, while the standard, according to Denis, Marcet, Berzelius, and Lecanu, never exceeds 8 per cent. Had due allowance been made by Dr. Thomson for the large proportion of albumen which I have shown to be included in his colouring matter, it would have been found that the albumen, instead of being deficient, was considerably in excess. In No. 1, for example, the amount of albumen in the colouring matter was 6. 970, which, added to the albumen in the table, makes 11. 836 per cent. In No. 4, again, the albumen amounted to 8. 509, which, added to that in the table, makes 14. 914 in 100 parts.

The following deductions evidently proceed from the several facts and observations I have brought forward.

1. That the universality of the diminution of the quantity of water in cholera blood is extensively and satisfactorily confirmed.

2. That a moderate diminution took place in the salts of the serum in the individual instance Dr. Thomson examined.

3. That Dr. Thomson's *calculation* of the quantity of salts in other cases is inadmissible because the quantities vary in every case.

4. That the apparent excess of colouring matter in the blood is chiefly dependent on the mixture of albumen, owing to the defective process employed in the analysis.

5. That this admixture of albumen is especially liable to take place in examinations of cholera blood, owing to the bulk of the crassamentum, and the quantity of serum it retains.

6. That nevertheless the same process may give correct results on standard blood, owing to the comparatively minute quantity of serum retained by the crassamentum. A great apparent difference, therefore, in the results of a standard analysis, and of cholera analysis, performed in the same manner and by the same individual, is not a proof of correctness in the estimations of the ingredients in cholera blood.

7. That the quantities of fibrin and albumen obtained by Dr. Thomson were not below the natural standard.

Before I conclude, I wish to add a supplementary word respecting another branch of our chemical knowledge of this disease. I allude to the peculiar evacuations. Since the publication of my results in THE LANCET of December, 1831, and of my report addressed to the Board of Health, the *alkaline* character of the evacuations has been ascertained in innumerable cases. I have, however, met one case myself, and I have heard of a second, in which the alvine evacuations possessed an *acid* reaction. In several dissections which I superintended in London, the contents of the stomach were also generally acid. The cause of this is sufficiently obvious. The stomach and small intestines naturally secreting a free acid, and in considerable quantities, the serous and alkaline exhalation of cholera is insufficient to neutralize the natural acid in these situations. If the acid secretion has again been unusually profuse before the attack of cholera, all the alkali of the blood will be insufficient to neutralize it, and consequently the evacuations must retain acid properties. The exception, therefore, does not invalidate the test by which cholera diarrhœa may be distinguished from ordinary bowel affections.

I have also to notice the interesting fact of Dr. Christison having discovered urea in the serous exhalation on the brain of a person deceased of cholera in Edinburgh. I wait with great impatience the publication of his results in this inquiry. I may venture to predict, that he will leave nothing more to be learned of the chemical pathology of this curious and deeply-interesting disease.

COMPOSITION OF HEALTHY AND OF CHOLERA BLOOD.

LETTER FROM DR. CLANNY.

To the Editor of THE LANCET.

SIR,—I have read the critique which appeared in THE LANCET on the 5th of this month, and though I could enter most freely and most satisfactorily upon the points animadverted upon therein, I shall forbear, in my present position, from occupying valuable time by any remarks except the following, which being quite to the point as to my chemical investigations of healthy and of cholera blood, I trust you will, with your usual candour, give an early insertion of them in the pages of your valuable and independent journal.

You are perhaps aware, Mr. Editor, that Professor Thomson of Glasgow has favoured us with analyses of healthy and of cholera blood, an account of which appeared on the first of this month in the *Philosophical Magazine*. I shall submit analyses made by myself and by Professor Thomson to your readers, and leave the chemists and medical practitioners to form their opinion of the value of my humble services in the investigation of this new and most formidable disease.

	Healthy Blood.		Cholera Blood, No. 1.		Cholera Blood, No. 2.	
	Dr. Clanny.	Professor Thomson.	Dr. Clanny.	Professor Thomson.	Dr. Clanny.	Professor Thomson.
Water	75.6	78.4	72.0	66.1	64.4	67.9
Albumen	12.1	8.5	6.1	4.9	3.1	6.3
Fibrin	1.8	4.4	.6	.4	.6	1.3
Colouring matter	9.1	7.4	21.0	27.4	31.9	23.2
Salts.....	1.4	1.3	.3	1.2	.0	1.3

Albumen.—Professor Thomson states, in his analyses at page 335 of the *Philosophical Magazine*, the quantity of albumen wanting to make up the quantity in healthy blood to be, in No. 1, 3.6, and in No. 2, 2.2, but seems to conjecture that part might be absorbed by the cloth through which it was filtered. With me, at No. 1, .6, and in No. 4, .9, to make up the quantity in healthy blood.

Fibrin.—Professor Thomson states the fibrin in cholera blood No. 1 to be about one-tenth of that of healthy blood, the quantity of blood No. 4 to be nearly four times as great, yet only one-third of the quantity that exists in healthy blood. With me, No. 1 and No. 4, to contain only one-third of the quantity that I found in healthy blood.

Colouring Matter.—Professor Thomson, at page 357, states "that the great excess of colouring matter in cholera blood is no less remarkable than the deficiency of fibrin. If we make allowance for the albumen and salts still contained in the colouring matter, and take a mean of the quantity of colouring matter in Nos. 1 and 4, we shall find it to be little short of four times the quantity of colouring matter in healthy blood." With me, No. 1 to be 2¼ times, and No. 4 nearly 4 times the quantity of colouring matter, &c., in healthy blood.

Salts.—Professor Thomson finds the salts in healthy and cholera blood to be nearly equal. I find the salts in healthy blood to be nearly 4½ more than in cholera blood No. 1; and in No. 4 the salts are not present. Compare this with the solid contents of the dejected fluids in cholera cases.

It will readily be observed, that in the above statement of results by Professor Thomson and myself, there is some difference in the numbers, which must be attributed to the nature of the cases from which the blood was taken; besides, I dry the albumen at 160°, and press the fibrin, and dry it in the atmosphere, so that these substances may be estimated in a more accurate manner than formerly practised, as I have mentioned in the *Edinburgh Medical Journal*, from finding, that by considerable or long-continued heat, I can, in the former method dry * in the albumen * one half.

Taking these things into consideration, I am satisfied that the numbers which Professor Thomson and I have given, are as near as may be, and are satisfactorily corroborative as regards the nature of the disease in question.

I need scarcely remark, that in the above tables I add the *free carbon* to the colouring matter, and the reason why I adhere to the plan in my work, of giving these substances in a separate form, as observed in my tables, is because I consider that they exist in the blood in that state, and I have no doubt but all unprejudiced persons will agree with me on that point.

My work is now before an enlightened community, and will be judged according to its merits, and not by what any individual may now say upon my method of analysis, or upon the accuracy with which my experiments were performed, except the contrary be proved by experiments.

After all that has been written in the above-mentioned critique, I beg to remark that my method of analysis and accuracy of experimenting in the investigation of the free carbon of the blood are unimpeachable, and that the hint which I gave towards the conclusion of my work, as to the gases contained in the colouring matter of the blood was only an after thought, and that they were not considered as forming gaseous constituents of healthy or cholera blood, more than the cinder which I found in the platinum vessel along with the free carbon, and cannot be estimated as of any importance as regards the nature of the blood circulating in the veins of healthy persons, or of individuals affected with the cholera.

No person who has performed experiments upon blood but must be sensible of the disagreeable smell emitted when the colouring matter is submitted in a crucible or platinum box to a high temperature, and in cholera cases this empyreuma was abundant and highly offensive. This remark does not apply to experiments with the blood of the horse, as I have lately found in prosecuting my experiments upon the blood of that animal; but of this more hereafter.

I cannot enter further upon this critique except I become personal, and may perhaps be induced to cut deeper in a certain quarter than for my own sake and for the sake of your readers I ought to do. Though I live at a considerable distance from London, I know more facts, as connected with the subject of cholera, than some persons suppose I can possibly do.

I never saw the constituents of blood according to Michaelis, being perfectly satisfied with those of Berzelius, Brande, Thomson, and my own.

The two latter I have given above. In the critique the carbon contained in carburetted hydrogen gas, and carbonic acid, which I mention, appear to be forgotten! So much for*

As to the severe animadversions on free carbon of blood, allow me to say that we are compelled to admit its existence from what

* Blanks in the copy.—Ed.

* The seal has here obliterated a word.—Ed.

we know and acknowledge as connected with the process of respiration, independently of the changes induced upon the blood, from impeded respiration generally, and my experiments particularly. I remain, &c.,

W. Reid Clanny.

Sunderland, May 12, 1832.

LITHOTRIPSY.

To the Editor of The Lancet.

Sir,—I am requested by Baron Heurteloup to transmit for insertion, in your valuable journal, a digest of his memoir on lithotripsy lately read before the "Institut de France." The improvements which are detailed cannot fail, I think, to interest your readers, as they are so important as to give a new aspect to the science. I have taken the liberty of incorporating with the text the voluminous notes of the Baron, and I beg to state that if any of the terms employed should seem obscure, a full explanation of them will be found in a work lately published by him on the "Principles of Lithotrity." The efficiency of the new instrument was demonstrated on artificial calculi, introduced into the bladder of subjects in the presence of the commissioners appointed by the "Institut" to examine into its merits. M. Dupuytren was the chairman of these gentlemen, and he, as well as his colleagues, after comparing the operation of the instrument with that of the perce-pierre in the hands of another surgeon, remained convinced that the vibration of the former is much less than that of the latter. The Baron states that accident first suggested to him the idea of his "Percuteur." Having, in one instance, forgotten his drill-bow, his only resource was to strike the drill with a hammer; and in this manner he succeeded in breaking a calculus of moderate size. He afterwards frequently resorted to the same means, especially in breaking the coque or shell after the operation of the "eviseur à forceps." I have the honour to be, &c.

D. O. Edwards.

9, Duke-street, Westminster,
May 19th, 1832.

ON THE NEW METHOD OF DESTROYING

VESICAL CALCULI,

BY MEANS OF PERCUSSION.

WITH EIGHT SUCCESSFUL CASES.

From a Memoir

By Baron Heurteloup.

The art of removing the stone from the urinary bladder without incision, hitherto designated by the term *Lithotrity*, may, if we reflect on the time which has elapsed since its first invention, be considered as of recent date; but if we consider the labour which has been bestowed on its cultivation, and the astonishing improvements it has undergone, we shall probably be disposed to allow that it is better entitled to the honours of antiquity than many others.

The modifications which have been admitted in this operation make the original name no longer applicable, for its derivation from λιθος, and τετρᾰω, or τερω, imply that its action is to *bore*, not to *break*. The term *lithotripsy*, from λίθος and τρίβω τρῖψις, will, I think, be acknowledged as more appropriate to the nature of the operation. Certain writers, having a cacoethes for coining words, have had no difficulty in forming grandiloquent names for the different stages of the lithontriptic process; such as *lithocoilancy*, from λιθος and κοιλαινω, and δοχος. Other equally barbarous terms have been manufactured from λιθος and θλαω, as well as λιθος and κοπτω. But these tasks may properly be resigned to Dr. Pangloss. The word lithotrity, however, does not now apply with any exactness to the general scope of the process, and may perhaps be advantageously supplanted by the term "*lithotripsy*." Having had the good fortune of contributing a large part of the materials with which this art has been raised to its present state of perfection, I am anxious to deposit, in the archives of the Institut, this further fruit of my labours, which I hope will afford a convincing proof that perseverance will occasionally enable us to attain results such as could not be anticipated by any person influenced by prejudices.

Hardly are we recovered from the surprise excited by the discovery of a means of wearing away the stone from the bladder by slow degrees, than the possibility of rapidly destroying these bodies by percussion, is practically demonstrated.

Had our first exertions not been shackled by long-existing prejudices, the method of percussion would probably have first suggested itself as the most natural means. It will readily be conceived that the more minute the particles into which a calculus is reduced, the more easily will they be evacuated, and the more prompt will consequently be the patient's relief. The means of breaking down calculi, immediately and completely, into powder, have steadfastly been sought for, but all attempts towards this end have hitherto been abortive.

The idea of effecting an *immediate and complete pulverization* was of necessity given up, and a more tardy process of fracture was next thought of.

There are *three* modes in which fractures may be produced. The first and most

simple of these is, to place the stone upon a fixed plane, and to strike it with a moveable plane, ex. gr., a hammer and an anvil.

The second in point of rapidity and simplicity is to place the stone between long moveable planes, and thereby compress and crush it.

The third mode, and the most tedious and ineffective, is the gradual consumption of the stone by a process of attrition.

I think that any person when called upon to break a stone into powder on the outside of the bladder, would naturally have recourse *first* to percussion, as the most efficacious and rapid way; *then* to crushing; and *last of all* to progressive trituration.

Such is the course which the surgeon would naturally have pursued, but the power of established theories, operated to warp his judgment. In fact, nature having placed limits to the size of the instruments to be employed, the practicability of *violently* breaking calculi in the interior of the bladder was not likely at first to suggest itself, for it would appear next to impossible to invest such instruments of contracted bulk with sufficient resistance to allow of the powerful actions of *percussion* and *crushing*.

The union of *power*, *lightness*, and *delicacy*, requisite for seizing calculi in the bladder might naturally appear a desideratum not easily to be accomplished. Concluding, therefore, that the combination of *great force* and delicacy of operation was perfectly impracticable, the inventor was naturally led to the original lithotritic means.

Fully impressed that a sudden collision of forces could not safely occur in this organ, and that the process of destruction must be slow, it was thought necessary to construct one instrument to hold the stone, and another to act upon it. And if, before the construction of his set of instruments, the inventor thought the powerful means alluded to ineligible, he was much more likely to think so afterwards, for the greater the number of pieces in a tube of given dimensions, the less is the strength of each, and the greater the feebleness of the entire apparatus.

Having once begun his course, the lithotritist soon meets with incidents that show him, even whilst using the drill, that the operation might often be much accelerated by crushing small stones or fragments with the claws of the instruments. With the instruments employed to produce excavation or perforation, percussion was made, and often succeeded in breaking the calculus. These incidental facts, of which experience soon supplied a great number, led the surgeon to perceive that force would be a valuable acquisition, since the diameter of the instrument allowed of its development, and hence the more expeditious plan of crushing had its birth.

The application of this system gave to lithontriptic agents the important property of making the same parts of the instrument effect both the seizure and the fracture. This improvement also, by diminishing the number of pieces in the tube, rendered it less complicate, and increased its power.

The abandonment of the practice of using the hand of an assistant in the operation, and substituting for it the point d'appui of the rectangle-bed, was a circumstance favourable to the application of more powerful instruments. This enabled the surgeon to effect his purpose with great steadiness, and contributed to place the surgeon and the patient in the best possible position with respect to each other.

The agency of such a coarse and powerful instrument as a hammer, would appear incompatible with the delicacy of manœuvring necessary in the interior of the living bladder, and it might be supposed that many trials would be made previous to a successful adoption of the system of percussion.

It was indeed after a train of speculations, such as I have described, that I was led to contemplate the application of such an instrument to the purpose.

After having had the good fortune to devise and bring into practice the apparatus called "*eviseur à forceps*," which accelerated the old process, by adding the principle of excavating; after having contrived an instrument which has proved effectual in the *crushing* of calculi, and denominated "*brise-coque*;" after having ascertained the best respective position, both for operator and the patient, and facilitated the movement of the latter by means of the rectangle-bed; after having invented a secure fulcrum for the instrument, while in the bladder, in the *point fixé*, attached to the foot of the bed; after finding all these designs approved of, and imitated, by highly respectable practitioners, and after proving their efficiency by numerous successful operations; and after having my labours repeatedly crowned with the high sanction of the Institut, I have once more the happiness of presenting for your examination a novel apparatus, in which the agency of percussion is resorted to.

I have called the new instrument the "*percuteur courbe à marteau*." It is made of steel, and is about fourteen inches long. For the sake of convenient description, it may be distinguished into that part which is contained in the urethra and bladder, and that which is external. The former has the appearance of a large sound, straight for about eight inches of its length, and presenting at its extremity a curve equivalent to the

quadrant of a circle of which the radius is *two* inches. This curved extremity separates into two portions by a section at right angles with the straight part of the sound. The straight part is composed of three pieces, two of which are lateral, and the third internal; the former are a continuation of the external portions of the curve, the latter of the internal or anterior part. The terms external and internal relate here to the convexity and concavity of the curve. Now since the true external portions are firmly secured in a square piece of steel forming the "*armure*" of the instrument, and since the internal portion is moveable, it may be drawn forward; a hiatus is thus formed like the beak of a bird by which the calculus is grasped. The pulverization of the stone is effected with the aid of a hammer. The outer part of the instrument is smartly struck with the hammer, carried without loss of power through the steel rod, and imparted to the foreign body sought to be destroyed.

The mode of proceeding in percussion is this:—I place the "armure" of this instrument in the point fixé, and make it secure by means of a wedge-shaped piece of steel. With this provision, powerful blows may be dealt on the external part of the instrument, without exciting any vibration in that part which is within the bladder.

By means of this instrument a vesical calculus, however hard, is broken to pieces as soon as seized, indeed it is difficult to say which is most remarkable—its facility of prehension, or energy of comminution—and, strange to say, the size and hardness of a stone may for the future be considered only as trifling obstacles to the success of an operation. The dispatch which may now be ensured in the destruction of stone is a most important advantage, and forms a striking point of superiority in the new apparatus. The history of a few cases will, I think, contribute to establish the character of the "operation by percussion."

Mr. K., of Liverpool, 68 years of age, after suffering the torments of stone for *three* years, consulted me. Finding a small calculus, I thought it advisable to employ the *perce pierre* of Messrs. Leroy and Civiale. I was prevented seizing the stone with this instrument by "an anomalous fleshy prominence," which, by protruding between the claws of the instrument during its depression towards the bas-fond of the bladder, was necessarily pinched. After two fruitless attempts, I was convinced that this instrument could not be employed without lacerating the fungous or hernial mass which was protruded in my path. This excrescence I considered to be either a fibrous mass resulting from arectal fistula, which opened externally by a sinus terminating near the anus, or from the enlargement of the third lobe of the prostate gland. I employed the percuteur à marteau, and in *four* applications of three minutes each, I enabled the patient to void the detritus of a calculus which must have weighed *four* drachms, and which was composed at its nucleus of lithic acid, and at its exterior of the mixed phosphates.

Mr. C., of London, 48 years of age, suffered with symptoms of stone for *four* years. I ascertained with a sound the presence of *two* oval-shaped calculi, smooth, and composed of lithic acid, apparently sixteen or eighteen lines in diameter. The "percuteur courbe" was employed, and in six applications, of from *three* to *four* minutes each, the patient was completely relieved.

Mr. Rogers, 64 years of age, residing at Limerick, after having had the stone about a year, came to London. I sounded him and felt a calculus, which I estimated to be 16 or 18 lines in diameter; it was of a flattened oval shape, and moved freely in the bladder. In four applications of the "percuteur courbe," lasting the usual time, the whole of the stone was removed.

Mr. G., 62 years of age, had had stone for eight or ten years. He requested a surgeon in London to perform lithotrity upon him. The surgeon consented, and made use of the "perce pierre," but the stone was not laid hold of, and the patient, tired with fruitless attempts, consulted me. I waited a month, that the bladder might recover its former tranquillity, and that the catarrh of the organ might cease. I then employed the "percuteur courbe," and in *six* applications of the usual duration, a smooth, oval-shaped, lithic acid calculus of ten lines diameter was destroyed and brought away.

Mr. O., 63 years old, residing in Sussex, about 60 miles from London, had experienced for upwards of four years all the sufferings attendant upon stone in the bladder. I was called into consultation by Mr. Dodd and Mr. Caudy, his surgeons. I found a smooth, oval, uric-acid calculus of from 20 to 24 lines in diameter. In *seven* applications of the percuteur, and *two* of the brise-coque, Mr. O. was restored to perfect health.

Mr. Goodwin, of London, ætat. 66, was afflicted with stone *three* years ago, and underwent lithotomy, which was successfully performed by Mr. Brodie. A year after the operation he had a return of his former symptoms. He then resolved to have recourse to lithotripsy. He consulted a surgeon in London who undertakes this branch of surgery. This gentleman proceeded to operate with the perce-pierre, but he did not succeed. The stone was not caught, al-

though *three* several trials, of three quarters of an hour's duration, were made. I was then consulted, and Mr. Goodwin was relieved of his stone in four applications of the "percuteur."

Mr. Wells, 48 years of age, residing about 70 miles from town, had suffered from the stone for four years. When he came to me I found a large lithic-acid calculus in his bladder, about 24 lines in diameter. Notwithstanding a considerable catarrh of this organ, I thought it right to commence operating; and in *five* applications of the "percuteur," and one of the brise-coque, I was enabled to get quite rid of the stone.

Mr. F., the president of the College of Carlow, 62 years old, had suffered from lithiasis for eight years. He consulted Mr. Crampton, surgeon-general for Ireland, who was polite enough to recommend him to my care. I sounded the patient, and discovered a calculus of very large dimensions, which was, however, tolerably moveable in the bladder; it seemed of an oval shape rather elongated and situated transversely. The sensibility of the patient was very acute. In *seven* applications this stone was completely destroyed and evacuated.

These operations were performed in the presence of Sir Astley Cooper, Sir Anthony Carlisle, Messrs. Brodie, Copeland, Samuel Cooper, Green, Hamilton, C. Hutchison, Aston Key, North, Swann, Travers, Vincent, &c. &c. &c. I was also fortunate enough to be enabled to operate with the "percuteur," in the presence of M. Magendie.

If the details of these cases are well looked into and considered, and the instrument itself carefully examined, and a fair trial given to it, the following conclusions will be established.

1. That the most powerful means, viz. percussion, that could possibly be devised for pulverizing calculi, under any circumstances, have been rendered safely applicable in the interior of the bladder.

2. Lithotripsy cannot henceforth be deemed inapplicable to large, flat, and oval calculi. As trophies of the prowess of the "percuteur," I beg to present to the "Institut" the fragments of stone voided by Mr. Wells; they fill a round box of 21 lines diameter, and 11 in breadth. I also present the model of Mr. F—'s stone, which I obtained in the following manner. I succeeded in filling exactly a cylindrical tin box, with the fragments evacuated, and afterwards formed a wax cast of the interior of this box. This cast I modelled into an oval shape. I had ascertained by my instruments the diameter of the stone, and knowing this, there was not much difficulty in approximating to the size and shape. This cast measures five inches, all but one line, in its smallest circumference. If now we reflect, that in this case the instrument was applied *seven* times, each application lasting about *three* minutes, we may say that the aggregate duration of the entire operation was only *twenty-one* or *twenty-two* minutes; and it is well known that lithotomy, in cases of large stones, has often continued longer.

3. The continuance of this operation need now never exceed a few minutes. It is only requisite to examine the "percuteur" to feel convinced that it cannot be otherwise, for it may be opened and closed 160 times in a minute; and its power will be seen to be irresistible.

4. Cases in which straight instruments cannot be introduced into the bladder, need no longer be considered out of the province of lithotripsy.

5. Frequent renewals of the operation are now prevented, even in cases of voluminous calculi, as is demonstrated by the history of previous cases.

6. Disease of the bladder is a comparatively less formidable circumstance, for the cases of Messrs. Rogers, Goodwin, G——, and C——, with severe affections of that organ, were relieved as promptly nearly as those of the most healthy persons.

7. The chance of disease of the bladder succeeding the lithotriptric process are in the direct ratio of the time consumed, and are consequently diminished by the greater expedition of the percutient method.

8. Those cases which were peculiarly difficult on account of the stone not being situated in the axis of the *straight* instrument, are now easily managed, as is shown by the cases of Messrs. Goodwin and F——, in which the perce-pierre was useless for that reason, and the "percuteur" successful.

9. From the case of Mr. R——, where unsuccessful attempts were made with the perce-pierre, we may conclude that an enlargement of the third lobe of the prostate is not an insurmountable obstacle, wherever the percuteur may be had recourse to.

10. The influence of the improved method is also felt in cases where several small stones exist, for their destruction is speedily effected, even after the failure of the perce-pierre.

11. Even the ill effect of contraction is in a great degree obviated, on account of the small space which is adequate to the manœuvring of the percuteur; and we can make the bladder yield without danger of injuring it.

12. The evacuation of the bladder too is much facilitated, for the shape of the fragments resulting from the percuteur, is more favourable for their expulsion.

13. Less pain is inflicted, and less danger incurred by the use of this instrument than by the former ones. During the operation the patient is completely free from any painful sensation, and when the instrument is fixed immoveably on the proper fulcrum, not the slightest vibration is produced by the blows of the hammer.

These are the conclusions to which this newly-invented instrument, and the system it represents, give rise. They may, perhaps, be deemed of sufficient importance to deserve the attention of the "Institut," who have already expressed an interest in the invention of a new method of destroying urinary calculi. I believe this instrument has no resemblance to any hitherto constructed, and I trust it will be looked upon as an important step in the improvement of the science.

ON THE

CAUSES, SYMPTOMS, AND MORBID ANATOMY,

OF

SIMPLE AND MALIGNANT STRICTURE OF THE RECTUM.

BY

FREDERICK SALMON,

Senior Surgeon to the General Dispensary, Aldersgate Street.

(AS DESCRIBED IN A LECTURE DELIVERED AT THE INSTITUTION, APRIL, 1832.)

GENTLEMEN,—When I had the pleasure of meeting you last, I exhibited to your notice an example of what I termed *simple* stricture of the sigmoid flexure of the colon, and endeavoured,—by contrasting the early symptoms of the case, and those which occur during its progress, with such as usually accompany malignant disease,—to show that it was not one of the latter character. By comparing the morbid anatomy of the two descriptions of disease, I also illustrated the same position.

Now the like circumstances will occur in the rectum, and if we look to the morbid anatomy of stricture of this part, which is the subject I shall more particularly allude to on the present occasion, we shall find that in its early stages there is very little deviation from the healthy appearance. This is easy of explanation when we consider, that most descriptions of stricture are, in all probability (at their commencement), simply spasmodic affections dependent upon some particular circumstance giving rise to local irritation. Now this spasmodic action necessarily disappears with the vital powers, hence you may fail in discovering any disease after death, although the most unequivocal symptoms of stricture appeared during the patient's existence.

The first change which takes place in the rectum may be well elucidated, by referring to a fact with which most of you must be familiar. In examining the bodies of persons who have died of diseased lungs, it is not an uncommon circumstance to find the lower part of the colon and the rectum contracted to their smallest natural extent, and which may be mistaken for disease. The parts, however, are easily dilated, by inflating them with air, or distending them with water, proving, consequently, that there is no organic alteration.

The first alteration that takes place is in the mucous coat of the bowel, which is thrown into large folds, as is shown in this preparation. This, however, is no disease of the part, but results simply from the fact of the mucous coat not being indued with contractile power equal to that which the muscular possesses. Thus the latter naturally assumes the appearance you here see represented. As disease advances, irritation is set up in the bowel, in consequence of which, deposition ensues, not in the muscular coat, nor in the inner tunic, as some writers have represented, but in the cellular tissue which connects these two parts. This fact is incontestably proved in the preparation I now show you. Here is, as you perceive, an enormous extent of thickening, so that the canal of the rectum at six inches from the anus is nearly obliterated; even water will scarcely pass through it; yet there is no disease of the inner coat of the bowel. The individual from whose body this preparation was taken, did not die of the disease I now show you, bad as it is, but from an affection of the lungs. Indeed the disease of the rectum was not discovered until after the patient's death.

Such is the *ordinary* appearance of simple stricture; but at other times inflammation is set up in the rectum, and ulceration takes place in various parts of the mucous surface, as is here shown. In this instance there is contraction and deposition, together with destruction of a portion of the mucous coat of the intestine. In extreme cases, adhesions, more or less extensive, according to the period of the existence of the stricture, form between the rectum and uterus, or vagina, or between the bladder and the rectum. Here are two preparations admirably illustrating these facts. Sometimes it unfortunately happens, if the case be not attended to before it is much ad-

vanced, that ulceration takes place of that part of the rectum and the vagina, which are in contiguity, and thus a communication is formed between the anterior and posterior passages, as is here exhibited. It is really surprising how serious an extent of disease will occur in the rectum without vitally affecting the individual. Communications between the bladder and the rectum in simple stricture are not common, but I have seen them more than once; and I am acquainted with an individual who for more than two years has passed a considerable portion of his faeces in this manner. They always come away in a liquid state before he voids his urine. The suffering he endures at these periods is indescribable, yet, from his general health and appearance, you would not consider that he laboured under a disease of any moment.*

It sometimes, though *very rarely*, happens, that you have lymph thrown out, forming bands in the rectum, and if you do not carefully examine the bowel, you may mistake these for stricture of the gut itself. I have a patient now under my care who has been on the books of this charity for four or five years, in whom the septa at the lower part of the rectum are extremely numerous; and I have been endeavouring for some time to induce her to allow me to divide them, but to no purpose; in her case, upon introducing your finger, you discover the septa extending from the anterior to the posterior parts of the rectum leaving a passage above and below, the largest of which channels will now barely admit of the introduction of a tobacco pipe. You may say that these are very extraordinary cases, and I do not deny that they are so, but still they are facts which I know to be true, and they verify my position as to the extent to which disease will go on without producing any great derangement of the general health.

Now these are the usual appearances when the disease is situated high up, beyond the reach of the finger, or, I would say, above the last curve of the rectum. I have shown you that there is little ulceration of the mucous coat when the stricture is high up; but as it approaches the anus, you almost invariably find the reverse. The preparation I now show you, which was taken from the body of a man who accompanied the expedition to the North Pole, illustrates this fact; and I believe the disease in this individual to have arisen solely from his frequently sitting upon ice, the extreme cold producing spasmodic action of the circular fibres of the rectum, which, as you know, are more prevalent near the anus than in any other part of the bowel. It is for this reason that you will generally find circular contractions within reach of the finger. This circumstance has led to an error that has sacrified many a valuable life. Medical men, finding circular stricture within reach of the finger, have expected that they should meet with the same description of contraction in the upper parts of the bowel. Such is not the case. At this situation the obstruction is produced by a partial obliteration of the cavity, from deposition between the coats in the direction of the longitudinal fibres. As the stricture near to the anus progresses, irritation is set up in the soft parts around the entrance of the bowel, and small abscesses, forming fistulæ, follow, as is here shown. Here is another instance of circular contraction within reach of the finger, wherein the cavity of the intestine is almost obliterated. I had not an opportunity of seeing the patient from whom this preparation was taken during his lifetime, but I attended, in the preceding instance, from its commencement, as likewise in the case I now exhibit, in which the cavity of the bowel will not, you perceive, admit a common tobacco pipe.

Now no one of the individuals from whom these morbid parts were taken, died from the disease of the rectum; and it is a fact that patients are very seldom carried off by simple stricture, which circumstance, again, has in all probability led to the belief, that diseases of the rectum are much less common than they in reality are. I have noticed a fact which occurred in the cases of these individuals many, many times, and that is, that persons do not die from disease of the intestine, but from consumption. Here I will take the opportunity of setting myself right with many members of the profession, for whom I entertain the sincerest regard, who have attributed to me opinions I never advanced, to wit, that disease of the rectum *causes* phthisis. I have never stated any such thing. If you will peruse what I have written on the subject, you will find, that all I say is, that I have seen the two diseases occur in conjunction, and that the affection of the rectum precedes the disease of the lungs. On this latter point I am at issue with those who state that fistula is the *consequence* of disease of the lungs. I believe the reverse to happen, and I say so from experience, for I have known repeated instances in which, upon the removal of the fistula that occurred as a consequence of circular contraction of the rectum, structural disease of the lungs has supervened. But however this may be, when the disease

* A case was here mentioned in which an artificial anus had been for many years formed in the groin, the particulars of which are suppressed in this report of the lecture.

is near the anus, you will generally find fistula and external piles, but if the obstruction be beyond the reach of the finger, you will more commonly have prolapsus. This fact will be clearly accounted for when you reflect, that if the constriction be near the orifice, there cannot be sufficient of the mucous membrane below the stricture to form a prolapsus.

Now such are the appearances usually found in what I would denominate *simple stricture*; but if we go to the morbid anatomy of *malignant* contraction of the rectum, we have such a state of things as is really astonishing. Here is a specimen of this description of disease, in which the whole of the lower, and a great portion of the second curve of the intestine, are destroyed, together with a part of the bladder. Here is another preparation, exhibiting carcinomatous disease of the rectum, where the entire sphincter, together with a great portion of the extremity of the bowel, is eaten away. It is the character of malignant disease, that it will destroy any part with which it comes in contact. You have scrofulous disease of the rectum, though I believe, as far as experience enables me to judge, this description of contraction is very rare. The specimen I show you, illustrating this fact, was taken from the body of a patient who recently died, and whom I first saw about nine months since. Upon examining the rectum with the finger at that period, I found the extremity of the bowel hardened and tuberculated. Looking further, I discovered the obstruction two inches from the orifice. He was then passing blood and matter with his motions, the inguinal glands in both groins were greatly enlarged, and his general health altogether was much disturbed. He ultimately died of scrofulous disease of the lungs. In this instance you see the mucous membrane at the anus, and for three or four inches up, is totally destroyed, and that it is ulcerating in small patches in the upper curves of the rectum.

You read of *syphilitic contraction* of the bowel, but I have no example of it to show you, neither have I ever seen an instance of the disease. I have seen many cases, especially in women of the town, where there was a vast mass of enlarged glands around the external verge of the anus; this affection is by no means an uncommon one, but I have never seen anything like syphilitic disease of the intestine itself.

So much for the morbid anatomy of simple and malignant contraction of the rectum. Now I do not place this before you as being anything like a complete elucidation of the subject; a great deal more might be said respecting it, yet I think I have said sufficient to enable you to distinguish between that description of contraction which I term simple, and malignant disease of the intestine.

I will now say a few words concerning the *situation* of stricture.

It is a commonly-received but very erroneous opinion, that stricture of the rectum is always within reach of the finger, and, certainly, malignant contraction, as well as the most difficult forms of simple stricture (I allude to circular contraction in particular), usually occurs at the extremity of the bowel. But if you refer to the anatomy of the part, you will readily perceive why stricture may occur in any part of the intestine. By the model of the pelvic viscera I now show you, you perceive that the rectum is divided into three curvatures. Here you have the first, here the second, and here the third curvature of the bowel.* Now when the bowels are habitually constipated, fæces are apt to accumulate at the angles of these curvatures, and to set up irritation, giving rise to chronic inflammation, from which, deposition in the cellular tissue connecting the mucous and muscular coats, results, producing a permanent thickening. Hence obstructions are more frequently found at these than at any other portions of the rectum. Here are various examples of the stricture occurring in different parts of the bowel, but all beyond the reach of the finger, and some to such an extent as nearly to obliterate its canal. Now malignant disease commences, in all probability, in the mucous glands, which, as you know, are far more prevalent near the anus than in any other part of the rectum; hence, it is this description of contraction that is so frequently found near the orifice. It should seem as if nature increased the glandular structure near this part, to guard against local irritation from any accumulation of indurated fæces, by lubricating the bowel and softening its contents.

Next, as to the causes of stricture.

If strictures, as I have told you, are, in their commencement, the result simply of inordinate action of the muscular coat of the bowel, it follows, as a necessary consequence, that they may be produced by any circumstance giving rise to undue action of this part, and, if I were to be asked what habit I consider has, more than any other, induced contraction of the rectum, I should reply, the frequent use, or rather abuse of purgative medicines. Indigestion, acrimonious substances lodged in the bowel, habitual costiveness, or, in fact,

* Here was exhibited a beautiful model in wax of the pelvic viscera, constructed by Huhna Grande, by whom it was brought to this country.

any circumstance which promotes irritation in the canal, unquestionably may give rise to the disease. It may be produced from some peculiar formation in the intestine, which is sometimes unusually long, so that there are several lateral curvatures in the upper part of the bowel, in which the feculent matter accumulates and causes irritation. It may originate in tumours, as, for example, polypi; in a mass of piles; or in any preternatural growth in the bowel. A common cause of stricture will be found in some particular conditions of the sphincter muscles. You all know that the office of the external sphincter is to retain the contents of the rectum; but if the power of this muscle be too great, it prevents the easy evacuation of the fæces; hence it requires excessive straining to empty the rectum; this sets up the irritation, and causes violent contraction of the bowel, and, ultimately, permanent stricture. This condition of the sphincter varies. Sometimes you will find a complete separation between the external and internal sphincter, so that there is nothing to relieve the extremity of the rectum, by which the difficulty of passing the motions is materially increased. This peculiar formation of the rectum has been often mistaken for stricture, and, certainly, if you do not make a careful examination, you may readily fall into this error; it is, however, no disease of the bowel, but arises simply from the internal sphincter not uniting, as it usually does, with the external. At other times you find the *external* sphincter remarkably broad and thick. There is a great similarity between this condition of the muscle, as giving rise to stricture of the rectum, and that which occurs in contraction of the orifice of the urethra. You will sometimes find the meatus externus extremely narrow, consequently offering a powerful opposition to the expulsion of the urine, and just the same effect happens in the rectum. This fact of contraction of the orifice of the urethra is not generally noticed by writers, yet it is of great practical importance, and one which you will very commonly find.

But the circumstances I have mentioned are not sufficient to account for the prevalence of stricture, which not unfrequently attacks many members of the same family. I have seen repeated instances of this fact. It should seem, therefore, that there is some common predisposing cause in which the disease originates. Mr. White stated, that he believed stricture very frequently resulted from a narrowing of the sigmoid flexure of the colon; and if you look to the position of the bowel, and the natural consequence that is likely to follow from an accumulation of fæces in this part of it, it is not improbable that his opinion was correct. Hence, if this narrowness of the part be consistent with birth, the individual may be born with a predisposition to stricture; and this I believe is frequently the case. Now we all know that a child usually resembles his parents in its features; and if the parent has a disposition to stricture of the rectum from this peculiar formation of the sigmoid flexure of the colon, I confess I see nothing irrational in supposing that the same similarity may occur internally, and is visible externally; and if this is admitted, the prevalence of the disease is in great measure accounted for.

And now with regard to the *symptoms* of stricture, and with which it is extremely important that you should be well acquainted.

I would remark here, that there is not, perhaps, any disease with which we are conversant, that is really so insidious in its early stages. A man has his bowels irregular, which he attributes to simple costiveness; he takes a dose of opening medicine, and finding relief, gives himself no further trouble about the matter. The same thing goes on from week to week, from month to month, and even from year to year, for certainly diseases of the rectum, when of a simple nature, are very slow in their progress, and still slower in producing any serious effect on the general health. It is extraordinary what an extent of time persons will go without passing an evacuation. Individuals have told me, that they have gone a fortnight, three weeks, and even a month, without having a stool. If you refer to the second volume of the "Transactions of the Society for the Improvement of Medical and Surgical Knowledge," you will read of a case of stricture at the sigmoid flexure of the colon, the subject of which lived the extraordinary period of fifteen weeks, without voiding anything from the bowels, and during a portion of that time scarcely any inconvenience was produced. The real nature of this case was not discovered until after the death of the patient, a lamentable mistake, which, I fear, too frequently happens. A person has stricture; immense quantities of opening medicine are given to relieve the constipation, but no good results; and ultimately he dies, the post-mortem examination, alone, proving, too late, the cause of his disease.

Another feature in the complaint is the condition of the stools, as regards their size and shape. Now these points are by no means conclusive. A man may have stricture to a serious extent, yet the evacuations may be in every respect well formed. Take, for example, the instance of stricture situated five or six inches up the bowel.

The feculent matter may slowly drip through the stricture into the healthy portion below it, and there become consolidated. Hence you may inspect the motions, and being deceived by their appearance, never suspect disease high up in the bowel. Again, their shape may be formed by passing over an enlarged prostate, or a displaced uterus, either of which circumstances may render them thin and flat. Patients commonly tell you, that their stools are flat, like ribands. There may be piles near the orifice of the rectum, and the evacuations passing over these, they assume some particular form. For these reasons, therefore, you must not place too great a reliance on the form and size of the motions. The difficulty of voiding them is a far more conclusive symptom of stricture, and you will find this feature more prominent when the obstruction is near to the anus. The power of the abdominal muscles in propelling the contents of the rectum is here lost, and the fæces are expelled by the contractile action of the *internal* sphincter, consequently if the stricture be situated near the orifice, this essential function is destroyed or materially lessened.

But as the contraction becomes more confirmed, you have irritation set up in the adjacent organs. If the stricture be high up, the feculent matter accumulating above it presses upon the fundus of the bladder, and prevents its entire distention, so that the patient is annoyed by a frequent disposition to void his urine. On the contrary, if it be near the orifice, you will more commonly have incontinence of urine. The neck of the bladder becomes irritated, by which the organ is rendered incapable of holding its contents. I have known many cases of incontinence of urine result from stricture of the rectum, which you will not be surprised at when you reflect on the contiguity of the parts. In the female the functions of the womb become disturbed. Sometimes the patient is irregular as regards the menstrual discharge; at one time there is amenorrhœa, at other times you will have menorrhagia. Many miscarriages result from this cause; the morbid adhesions formed between the uterus and the rectum, preventing the ascent of the former organ, it relieves itself by rejecting its contents. I have known this to happen over and over again. Muco-purulent discharges often take place from the vagina in the female, and from the urethra in the male; occasionally the individual has attacks of colic or spasm in the colon, and if the stricture be high up, nature sometimes accommodates herself to the diseased action, and makes an artificial rectum, if I may use the expression, in an enormous enlargement of the colon, as is here shown.

Now I shall claim your indulgence while I just refer to this person's case. The model which you here see was taken from a man now living, who was labouring under stricture of the rectum at about its union with the sigmoid flexure of the colon. The model was moulded about twelve months ago. When I first saw the patient, the enlargement of the colon was full three times as great as you here see it represented. I met the man from whom it was taken by accident, he was begging, and I was so struck with the peculiar expression of his countenance, that I inquired what was the matter with him. He told me that he had some disease in the bowels, and that he had been discharged from all the hospitals in this metropolis as incurable. Now I took some pains to ascertain whether he told me the truth, and I know that he was in three public charities, where his complaint was denominated "internal abscess;" but it proved to be neither more nor less than an enormously distended colon. At the time I [illegible] him he was spitting matter from th[illegible] and appeared to be fast approachi[illegible] he, however, recovered, and [illegible] He had been a soldier in [illegible] Company's service; and I i[illegible] to procure him a pension, [illegible] pected he would have lived [illegible] He has left me his body, [illegible] to keep my own eye upo[illegible] sixpence a month, to call [illegible] himself; and I can assure [illegible] very effectual mode of lo[illegible] He seldom fails in paying [illegible] his sixpence is due.

Now reflect what must be [illegible] such a distention of the colon a[illegible] see; why, by pressing upon the k[illegible] will disturb their function, it will de[illegible] the digestive power, as likewise the action of the liver, and furthermore, by pressing upon the aorta it will prevent the free circulation of the blood in the lower extremities; hence the patient will be annoyed by cramps in the legs and feet, and, in extreme instances, by producing a violent determination of blood to the head, it is very likely to give rise to apoplexy.

I mention these circumstances to you, not because I consider them as the usual symptoms of stricture in the rectum, for I should be sorry if you supposed that I thought any such thing; but in order that you may see what may follow neglected disease of this part. It is not at all an uncommon feature for persons to be attacked with inflammation of the bowels or peritoneum, and you will find extraordinary cases on record, some of them of recent date, in which patients have died from obstruction of the bowels, resulting from stricture in the rectum, where this disease was not even

suspected during life, and would probably have never been known at all, but for the post-mortem examination. It often happens, that simple stricture of the rectum goes on to a most extraordinary extent, and the patient ultimately dies, not from the affection in the rectum, but from some other disease, which is the consequence of it.

In malignant contraction, however, there are characteristics by which you can always discover the disease in its early stages; in fact, no man who has a malignant affection in the rectum ever fails mentioning it; his suffering is so acute, that he is compelled to seek the advice of his surgeon, and that at a very early period; whereas common diseases of the rectum are often kept by patients to themselves. They do not experience much pain from them, and they would rather bear that than what they are pleased to call, expose their complaint. But in malignant affections of the rectum, [illegible] have the most intense pain in the part, [illegible] in the very earliest stages of the dis- [illegible]arting is felt in passing the stools, [illegible]nts will describe as feeling as if [illegible]any cuts over which you were [illegible]ron, or as if there were some- [illegible]be part with sand. Every [illegible] you have gushes of blood [illegible]s portions of it ulcerate, [illegible] stages of the disease you [illegible]t offensive discharge, just [illegible] as you usually see accom- [illegible]matous affection of the [illegible]portions of the mucous mem- [illegible]ly come away in the stools, [illegible]ction with these local symp- [illegible]ttation, the constitution equally [illegible] The patient soon becomes ema- [illegible]d and weak, and exhibits those fatal signs which a man who is an observer of diseased action, knows will generally be followed by death.

For these reasons you may easily discriminate malignant disease, but according to my experience, I confess to you that I do not believe malignant contractions of the rectum are so common as has been apprehended. You will read of scirrhous contracted rectum, but I have never seen many instances of what I should call *true* scirrhus of the rectum. I have shown you many cases of simple stricture of long standing, in which there was great thickening from deposition, but I think there is a distinctive difference between these appearances and those of true scirrhus. I mention this fact, in order that you may not set down all cases of stricture which may come under your observation in their early stages as being of malignant character, even though they are within reach of the finger, nevertheless you must not suppose that you will not have malignant disease beyond the reach of the finger, because, though it usually occurs near to the anus, recollect it may take place at any other portion of the rectum.

Such is the morbid anatomy, such are the causes, and such the symptoms, that occur in the progress of stricture of the rectum, whether it be simple or malignant. I will not detain you on the present occasion by going into the treatment of the disease, because it would occupy much more time than you would now be willing to spare. I will, therefore, defer it till the evening of our next meeting.

INJECTION OF

SALINE SOLUTIONS

IN EXTRAORDINARY QUANTITIES INTO THE VEINS IN CASES OF

MALIGNANT CHOLERA.

Communicated to the EDITOR *from the* CENTRAL BOARD OF HEALTH, *London.*

LETTER I.

SIR,—I conceive it to be my duty to let you know, for the information of the Central Board of Health, that the great desideratum of restoring the natural current in the veins and arteries, of improving the colour of the blood, and recovering the functions of the lungs, in Cholera Asphyxia, may be accomplished by injecting a weak saline solution into the veins of the patient. To Dr. Thomas Latta of this place is due the merit of first having recourse to this practice. He has tried it in six cases, three of which I have seen and assisted to treat. The most wonderful and satisfactory effect is the immediate consequence of the injection. To produce the effect referred to, a large quantity must be injected, from *five to ten pounds* in an adult, and repeated at longer or shorter intervals as the state of the pulse and other symptoms may indicate; whenever the pulse fails, more fluid ought to be thrown in to produce an effect upon it, without regard to quantity. In one of the cases I have referred to, 120 *ounces* were injected at once, and repeated to the amount of 330 ounces in 12 hours. In another 376 ounces were thrown into the veins between Sunday at 11 o'clock a. m., and this day (Tuesday) at 4 p. m., that is, in the course of 53 hours, upwards of 31 pounds! The solution that was used, consisted of two drachms of muriate, and two scruples of carbonate, of soda, to sixty ounces of water. It was at the temperature of 108 or 110.

The apparatus employed for injecting

The feculent matter may slowly drip through the stricture into the healthy portion below it, and there become consolidated. Hence you may inspect the motions, and being deceived by their appearance, never suspect disease high up in the bowel. Again, their shape may be formed by passing over an enlarged prostate, or a displaced uterus, either of which circumstances may render them thin and flat. Patients commonly tell you, that their stools are flat, like ribands. There may be piles near the orifice of the rectum, and the evacuations passing over these, they assume some particular form. For these reasons, therefore, you must not place too great a reliance on the form and size of the motions. The difficulty of voiding them is a far more conclusive symptom of stricture, and you will find this feature more prominent when the obstruction is near to the anus. The power of the abdominal muscles in propelling the contents of the rectum is here lost, and the fæces are expelled by the contractile action of the *internal* sphincter, consequently if the stricture be situated near the orifice, this essential function is destroyed or materially lessened.

But as the contraction becomes more confirmed, you have irritation set up in the adjacent organs. If the stricture be high up, the feculent matter accumulating above it presses upon the fundus of the bladder, and prevents its entire distention, so that the patient is annoyed by a frequent disposition to void his urine. On the contrary, if it be near the orifice, you will more commonly have incontinence of urine. The neck of the bladder becomes irritated, by which the organ is rendered incapable of holding its contents. I have known many cases of incontinence of urine result from stricture of the rectum, which you will not be surprised at when you reflect on the contiguity of the parts. In the female the functions of the womb become disturbed. Sometimes the patient is irregular as regards the menstrual discharge; at one time there is amenorrhœa, at other times you will have menorrhagia. Many miscarriages result from this cause; the morbid adhesions formed between the uterus and the rectum, preventing the ascent of the former organ, it relieves itself by rejecting its contents. I have known this to happen over and over again. Muco-purulent discharges often take place from the vagina in the female, and from the urethra in the male; occasionally the individual has attacks of colic or spasm in the colon, and if the stricture be high up, nature sometimes accommodates herself to the diseased action, and makes an artificial rectum, if I may use the expression, in an enormous enlargement of the colon, as is here shown.

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Now reflect what must be the effect of such a distention of the colon as you here see; why, by pressing upon the kidneys it will disturb their function, it will derange the digestive power, as likewise the action of the liver, and furthermore, by pressing upon the aorta it will prevent the free circulation of the blood in the lower extremities; hence the patient will be annoyed by cramps in the legs and feet, and, in extreme instances, by producing a violent determination of blood to the head, it is very likely to give rise to apoplexy.

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For these reasons you may easily discriminate malignant disease, but according to my experience, I confess to you that I do not believe malignant contractions of the rectum are so common as has been apprehended. You will read of scirrhous contracted rectum, but I have never seen many instances of what I should call *true* scirrhus of the rectum. I have shown you many cases of simple stricture of long standing, in which there was great thickening from deposition, but I think there is a distinctive difference between these appearances and those of true scirrhus. I mention this fact, in order that you may not set down all cases of stricture which may come under your observation in their early stages as being of malignant character, even though they are within reach of the finger, nevertheless you must not suppose that you will not have malignant disease beyond the reach of the finger, because, though it usually occurs near to the anus, recollect it may take place at any other portion of the rectum.

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SIR,—I conceive it to be my duty to let you know, for the information of the Central Board of Health, that the great desideratum of restoring the natural current in the veins and arteries, of improving the colour of the blood, and recovering the functions of the lungs, in Cholera Asphyxia, may be accomplished by injecting a weak saline solution into the veins of the patient. To Dr. Thomas Latta of this place is due the merit of first having recourse to this practice. He has tried it in six cases, three of which I have seen and assisted to treat. The most wonderful and satisfactory effect is the immediate consequence of the injection. To produce the effect referred to, a large quantity must be injected, from *five to ten pounds* in an adult, and repeated at longer or shorter intervals as the state of the pulse and other symptoms may indicate; whenever the pulse fails, more fluid ought to be thrown in to produce an effect upon *it*, without regard to quantity. In one of the cases I have referred to, 120 *ounces* were injected at once, and repeated to the amount of 330 ounces in 12 hours. In another 376 ounces were thrown into the veins between Sunday at 11 o'clock a. m., and this day (Tuesday) at 4 p. m., that *is*, in the course of 53 hours, upwards of 31 pounds! The solution that was used, consisted of two drachms of muriate, and two scruples of carbonate, of soda, to sixty ounces of water. It was at the temperature of 100 or 110.

The apparatus employed for injecting

was merely one of Reid's common syringes (the fluid being put into a vessel rather deep and narrow) with a small pipe so fitted that it might easily be introduced into an incision in the vein of the usual size that is made in bleeding. It may however be well to keep in mind that, in the event of the operation being frequently repeated, it may be advisable to inject by different veins.

I forbear at present to enter further into particulars; nor have we had sufficient experience to speak decisively upon the subject. I may however mention, that the idea of having recourse to the remedy in cholera occurred to Dr. Latta, from being convinced (which I am also) that the evacuations upwards and downwards, are in reality the serum of the blood; that it is the duty of the physician to replace it as speedily as possible, by injecting a fluid as similar to the serum as can be formed artificially, directly into the veins, which has been done here with wonderful, and so far as we can yet judge, excellent, effect. An immediate return of the pulse, an improvement in the respiration and in the voice, an evolution of heat, an improvement in the appearance of the patient, with a feeling of comfort, are the immediate effects. The quantity necessary to be injected will probably be found to depend upon the quantity of serum lost. The object of the practice being to place the patient in nearly his ordinary state as to the quantity of blood circulating in the vessels.

I have, &c.

(Signed) Robert Lewins, M.D.,
Fellow of the Royal College of Physicians, and Member of the Leith Board of Health.

Leith, 6, Quality-street, May 15, 1832.

To W. Maclean, Esq.,
Secretary Central Board of Health.

Letter II.

Sir,—I did myself the honour to address a letter to you lately, on the effect of injecting a saline solution into the veins of a patient labouring under cholera. We have not frequent opportunities now of trying this, which I denominate admirable remedy, as the disease is decidedly less frequent here: but I have seen it employed in two other cases in the course of the last two days, with the same excellent effect. Sixty ounces are generally thrown in at once, and repeated at the end of three or four hours. In a case to-day where I saw fifty-eight ounces injected (being the third time of performing the operation), the patient's pulse at the commencement was 180, very small and very feeble. She was excessively restless, with a feeling of great weakness and tormenting thirst. Before twelve ounces had been injected, the pulse began to improve; it became fuller and slower, and it continued to improve, until, after 58 ounces had been injected, it was down to 110. Before I left the patient (a woman), her condition was altogether amazingly ameliorated. There was a fine glow, and a slight perspiration on her face; the veins on the back of her hands were well filled; the restlessness was removed, the feeling of excessive weakness gone, and the thirst had ceased! The pulse was under 100, and full, free, and soft! Verily, Sir, this is an astonishing method of medication, and I predict will lead to wonderful changes and improvements in the practice of medicine.

I have addressed you upon the subject, as the organ, from your high official station, of most speedily and effectually disseminating a knowledge of the extraordinary facts referred to. It will of course give me great pleasure to enter further into particulars upon any particular point on which you may require information, in reference to the cases that have come under my observation.

I have, &c.

(Signed) Rob. Lewins.

6, Quality-street, Leith,
May 18, 1832.

In the hands of a man of ordinary dexterity, the common injecting apparatus alluded to in my last will be found to answer the purpose perfectly well; but if the practice I recommend is, as I hope it will be, generally adopted, it will, I conceive, be expedient to advise that a regular and perfect transfusion apparatus be used: at all events, to warn those who inject to beware of allowing air to get into the vein. The tubes, of course, must be filled with fluid as well as the pipe in the vein before commencing, and considerably more fluid than it is intended to use ought to be in the vessel from which it is pumped.

R. L.

ARSENIC IN ANGINA PECTORIS, &c.

To the Editor of The Lancet.

Sir,—I am not aware that arsenic has been given, or recommended, in the very formidable complaint termed "Angina Pectoris," and having some time ago given this powerful medicine to two patients labouring under alarming attacks of that disease, who had been under an ineffectual plan of treatment for some time previous, with great apparent advantage, I am induced to make the profession acquainted with the fact through the medium of your excellent journal.

M. Martinet is of opinion, that Angina is a neuralgic affection of the phrenic nerves; from this circumstance, and the known influence arsenic has in similar affections of the ischiatic and facial nerves, I was led to the administration of it in the present instances. The subjects were weak, attenuated men, between 50 and 60 years of age, with a tendency to asthma. They took ten minims each of the solution, in a little bread and milk, every eight hours between the paroxysms. In each case it was only necessary to continue the medicine about three weeks, and in neither has there been a relapse.

I have given this medicine in many cases of neuralgic headach with great benefit, nor does constitutional diathesis influence its good effects; for, in two cases lately, that were of a remarkably obstinate nature, and which had resisted almost every other mode of treatment, every symptom completely disappeared after the medicine had been taken for about *nine days*. The one case was that of a carpenter, *thick set, and of ruddy complexion*; the other was a *female, of delicate but healthy aspect*; the latter, in addition, took extract of stramonium at nights.

I am, Sir, your obedient Servant,

ROBERT JONES.

Strefford, near Ludlow, May 14, 1832.

ST. GEORGE'S HOSPITAL.

To the Editor of THE LANCET.

SIR,—The following account of some recent doings at our Hospital will, I hope, deserve a place in an early number of your journal; they indicate, in the strongest manner, the presence of most superlative and degrading carelessness, neglect, and nepotism.

A patient (Thomas Walker) was admitted into the Hospital March 28th, with calculus in the bladder. He was 68 years old, and of a countenance and constitution strongly indicative of great nervous anxiety respecting the operation, which was to have been performed on Thursday, 5th of April; but when that day arrived, it was deferred until the following Monday; and why? *Because Mr. Keate had cut his finger, and could not use the scalpel.* Monday came, and then *it was not expedient to operate, because Mr. Keate's cut finger had not quite healed*; but the operation was certainly to be performed on the following Thursday, *if the finger was well enough!* Thursday came, and the operation was performed; and I am bound to say, in justice to Mr. Keate, in a most masterly and scientific manner; and (*mirabile dictu*) the man has been doing very well ever since. The calculus was very large, and altogether the operation fully deserved the encomium which Mr. Brodie passed upon it, viz., that he had never seen one performed in a more scientific or skilful manner. I have, however, a few remarks to make on the above. So many patients have recently died in the Hospital after the operation of lithotomy, that, judging from every circumstance, very few expected the man to recover. And may I ask—

Had he died during the week of delay on the part of Mr. Keate, and had a Coroner's Inquest been held upon the body, what would have been the verdict recorded? It surely should not have been—"Died by the visitation of God." And yet I dare to say, the sapient jurymen would have starved for a week rather than impugn the character of Mr. Keate by bringing in his name in the verdict.

Giving Mr. Keate every due credit for his manual dexterity and science, may I ask—Whether Mr. Brodie is not fully his equal in these respects; and if so, why was this man made to undergo a week of extreme torture and suffering, because, forsooth, the President of the Royal College of Surgeons *had cut his finger?* Why was not Mr. Brodie requested to perform the operation on the day that it ought to have been done, since Mr. Keate well knew that he could not perform it? I pause for a reply to these questions.

On the evening of Wednesday, 18th April, a man was brought to the Hospital with hernia; and it being Mr. Keate's accident week, a messenger was dispatched to Albemarle-street for him. Mr. Keate was not at home; and the messenger went on to the College of Surgeons for him. And what answer do you suppose was returned?—"*That if the man could wait till midnight, Mr. Keate would then come and operate on him.*" When this answer was received at the Hospital, Mr. Babington was forthwith sent for, and he came and performed the operation. I need scarcely add, that the man died two days afterwards. On this latter case I shall make no remark: it needs none from me. Let Mr. Keate's memorable answer speak for itself.

Before I conclude, may I ask whether it is the intention of the governors of this Hospital to convert the board-room into an operating theatre. If not, why did Mr. Hawkins operate for nævus in the board-room a few days ago?

I am, Sir,

ALPHA.

St. George's Hospital, May 20th.

ROYAL INSTITUTION.

Friday, May 4, 1832.

MR. FARADAY ON VOLTA-ELECTRIC INDUCTION, AND THE EVOLUTION OF ELECTRICITY FROM MAGNETISM.

The power which electricity of tension possesses of causing an opposite electrical state in its vicinity, has been expressed by the general term induction, which may also be used with propriety to express the power which electrical currents may possess of inducing any particular state upon matter in their immediate neighbourhood.

Certain effects of the induction of electric currents have been already described: as those of magnetization; Ampere's experiments of bringing a copper disc near to a flat spiral; his repetition with electro-magnets of Arago's extraordinary experiments; and perhaps a few others. It was unlikely these could be all the effects which induction by currents could produce, especially as upon dispensing with iron, almost the whole of them disappear, whilst a great many bodies, exhibiting definite phenomena of induction with *electricity of tension*, still remain to be acted upon by the induction of *electricity in motion*.

It seemed extraordinary, that as every electric current was accompanied by a corresponding intensity of magnetic action at right angles to the current, good conductors when placed within the sphere of this action should not have any current induced through them, or some sensible equivalent effect produced. Stimulated by the hope of obtaining electricity from ordinary magnetism, I have at various times investigated experimentally the inductive effect of electric currents, and I have at length arrived at positive results; and have not only had my hopes fulfilled, but obtained a key which appears to me to open out a full explanation of Arago's electric phenomena, and also to discover a new state, which may probably have great influence in some of the most important effects of electric currents.

With the aid of coils or helices of wire, copper, iron, or silver, so arranged with respect to each other as to form two distinct circles; metallic contact being everywhere prevented, the one helix communicating with a galvanometer, and the other with a powerful battery, certain effects were rendered evident. When contact with the battery was made, a slight and sudden effect on the galvanometer was perceived, and a similar effect when the contact was broken. But whilst the voltaic current was continuing to pass through one helix, no galvanometrical appearances of any effect like induction upon the other helix could be perceived, although the active power of the battery was proved to be great, by its heating the whole of its own helix, and by the brilliancy of the discharge when made through charcoal. On a repetition of the experiment with a more potent battery, it was ascertained that the slight deflection of the needle occurring at the moment of completing the connexion, was always in *one direction*, and that the equally slight deflection produced when the contact was broken, was in *the other direction*.

These results led me to believe that the battery current through one wire, did in reality induce a similar current through the other wire; but that it continued for an instant only, and partook more of the nature of the electrical wave passed through from the shock of a common Leyden-jar, than from that of a voltaic battery, and, therefore might magnetize a steel needle, although it scarcely affected the galvanometer. This expectation was confirmed, for on substituting a small hollow helix, formed round a glass tube for the galvanometer, introducing a needle, making contact as before, and removing the needle *before the contact was broken*, it was found magnetized. When the battery contact was made first, then an unmagnetized needle introduced into the small helix aforesaid, and, lastly, the *battery contact broken*, the needle was found magnetized to an equal degree as at first; but *the poles were of a contrary kind*. When the unmagnetized needle was put into the indicating helix *before* contact of the inducing wire with the battery, and remained there till *after* the contact was broken, it exhibited little or no magnetism, the first effect having been nearly neutralized by the second. The force of the induced current upon *making contact*, was found always to exceed that of the induced current at *breaking of contact*; and if, therefore, the contact was made and broken many times in succession, whilst the needle remained in the helix, it came out at last, not unmagnetized, but a needle magnetized as if the induced current upon making contact had acted alone on it.

These experiments were again and again renewed with a different disposition of the wire, but the needle was deflected according to the same rule. When the wires were approximated to each other in the neighbourhood of the needle, the induced current was in the *contrary* direction to the inducing current. As the wires receded, the induced current was in the *same* direction as the inducing current. When the wires remained stationary, there was no induced current.

By numerous experiments I have shown, that collateral currents, either in the same

or opposite directions, exert no permanent inducing power on each other affecting their quantity or tension.

Neither evidence by the tongue, by spark, or by heating fine wire on charcoal, could be obtained of the electricity passing through the wire under induction. Neither could any chemical effects be obtained. This deficiency of effect is probably due to the brief duration and feeble intensity of the electric current.

Attempts to obtain similar effects to these by the use of wires, conveying ordinary electricity, were doubtful in the results. Still it does not follow that the discharge of ordinary electricity through a wire does not produce analogous phenomena to those arising from voltaic electricity; but as it appears impossible to separate the effects produced at the moment when the discharge begins to pass, from the equal and contrary effects produced when it ceases to pass, inasmuch as with ordinary electricity these periods are simultaneous, so there can be scarcely any hope that in this form of the experiment they can be perceived.

From all these facts, it appears to me evident, that currents of voltaic electricity present phenomena of induction somewhat analogous to those produced by electricity of tension, although many differences exist between them. The result is the production of other momentary currents, parallel or tending to parallelism, with the inducing current. By reference to the poles of the needle formed in the indicating helix, and to the deflections of the galvanometer needle, it was found in all cases, that the induced current produced by the first action of the inducing current, was in the *contrary* direction to the latter, but that the current produced by the cessation of the inducing current was in the *same* direction as I have already said. I propose calling this action of the current from the voltaic battery, *Volta-electric induction*. The properties of the wire after induction has developed the first current, and whilst the electricity from the battery continues to flow through its inducing neighbour, constitute a *peculiar electric condition* to be considered hereafter.

Having considered the effects of volta-electric induction, Mr. Faraday next inquired into the possibility of evolving electricity from magnetism. He procured a welded ring of soft round bar-iron, nearly an inch in thickness, and the circle being six inches in diameter. Three helices A were put round one part of this ring, and were insulated from the iron and from each other, and superposed in the same manner as in the first experiment. On the other part of the ring about sixty feet of similar copper-wire, in two pieces, were applied in the same manner, forming a helix B, which had the same direction with the other helices, but was separated from them at each extremity by about half an inch of uncovered iron. This arrangement was applied as in the former experiments. The last helix being attached to the galvanometer, and the first to a battery of ten pairs of plates four inches square, the galvanometer was immediately affected, and to a degree far beyond what occurred when, with a battery of tenfold power, helices without iron were employed; but though the contact was continued, the effect was not permanent, for the needle soon came to rest in its natural position. Upon breaking the contact with the battery, the needle was again powerfully deflected, but in the *contrary direction*.

Upon arranging the apparatus so that the helix B should be out of use, the galvanometer be connected with one of the three wires of A, and the other two made into a helix through which the current from the trough was passed, similar but rather more powerful effects were produced.

When the battery-contact was made in one direction, the galvanometer-needle was deflected on the one side; if made in the other direction, the deflection was on the other side. The deflection on breaking the battery-contact was always the reverse of that produced by completing it. The deflection on making a battery-contact always indicated a current proceeding in a direction opposite to that from the battery; but on breaking the contact the current was shown to be in the same direction. No continuance of the battery-current caused any deflection of the galvanometric needle.

Upon using the powers of 100 pairs of plates with this ring, the impulse at the galvanometer, at the making or breaking of contact, was so great as to make the needle spin four or five times round.

By a modified series of experiments with this ring, Mr. Faraday demonstrated that a feeble voltaic current sent through helix B and the galvanometer, did not affect the powerful current proceeding from the 100 pairs of plates connected with the helix. That when copper was introduced instead of iron, no effect beyond that of the helices are produced.

Similar effects were produced by *ordinary magnets*. Thus the hollow helix formerly described, had all its elementary helices connected with the galvanometer by two copper wires, each five feet in length; a soft iron cylinder was introduced into its axis; a couple of bar-magnets, each 24 inches long, were arranged with their opposite poles at one end in contact, so as to resemble a horse-shoe magnet, and then contact made between the other poles and the

ends of the iron cylinder, so as to convert it for the time into a magnet; by breaking the magnetic contacts, or breaking them, the magnetism of the iron cylinder could be destroyed or reversed at pleasure.

Upon making magnetic contact the needle was deflected; continuing the contact, the needle became indifferent, and resumed its first position; on breaking the contact it was again deflected, but in the opposite direction, and then it again became indifferent; when the magnetic contacts were reversed the deflections were reversed.

When the magnetic contact was made, the deflection was such as to indicate a current of electricity induced in the opposite direction to that fitted to form a magnet having the same polarity as that really produced by contact with the bar-magnets.

As it might be supposed that in all the preceding experiments it was by some peculiar effect taking place during the formation of the magnet, and not by its mere approximation, that the momentary-induced current was excited, the following experiment was made. A cylindrical magnet of the same size was substituted for the soft iron cylinder. One end of this magnet was introduced into the axis of the helix, and then the galvanometer needle being stationary, the magnet was suddenly thrust in; immediately the needle was deflected in the same direction as if the magnet had been formed by either of the preceding processes. Being left in, the needle resumed its first position, and then the magnet being withdrawn, the needle was deflected in the opposite direction. When the magnet is introduced, the needle of the galvanometer is deflected in a certain direction; but being in, whether it be pushed quite through or withdrawn, the needle is deflected in a direction the reverse of that previously produced. When the magnet is passed in and through at one continuous motion, the needle moves one way, is then suddenly stopped, and finally moves the other way.

Whichever end of the helix the magnet goes in at, and consequently whichever pole of the magnet enters first, still the needle is deflected the same way. On the other hand, whichever direction is followed in withdrawing the magnet, the deflection is constant, but contrary to that due to its entrance.

These effects are simple consequences of a law hereafter to be described.

All care was taken to guard against any direct action of the inducing magnet upon the galvanometer.

Mr. Faraday made several experiments with the great magnet of the Royal Society, and with striking results. Notwithstanding the great power of this magnet, if the contact was continued the needle resumed its natural position, being entirely uninfluenced by the position of the helix. But on breaking the magnet contact, the needle was whirled round in the opposite direction with remarkable force. With this potent agent numerous variations in the experiments already were made, but with effects always corresponding in kind with those resulting with lesser arrangements.

All attempts to obtain chemical effects, by the induced current of electricity, failed, although every precaution that could be thought of was adopted. Upon repeating the experiments with a loadstone belonging to Professor Daniell, and capable of lifting about thirty pounds, a frog was powerfully convulsed each time magnetic contact was made.

These experiments prove the production of electricity from ordinary magnetism. That its intensity should be very feeble and quantity small is not wonderful; for, like thermo-electricity, it is evolved entirely within the substance of metals, retaining all their conducting power. But an agent which is conducted along metallic wires in the manner described, which, whilst so passing, possesses the peculiar magnetic properties and force of a current of electricity, which can agitate and convulse the limbs of a frog, and which, finally, can produce a spark by its discharge through charcoal, can only be electricity.

The similarity of action, almost amounting to identity, between common magnets and either electric magnets or volta-electric currents, is striking, and furnishes strong reasons for believing that the action is the same in both cases; but as a distinction in language is still necessary, Mr. Faraday proposes to call the agency thus exerted by ordinary magnets,—*magneto-electric*, or *magnelectric* induction.

Tuesday, May 8, 1832.

IMPROVISING.

The Marquis de Moscati, an Italian refugee, delivered this evening an animated lecture on an institution celebrated in Italy, and entitled Arcadia, in the course of which he was led to give an interesting description of his initiation as an improvisatoire. When a paroxysm of inspiration came on, he felt the most extraordinary excitement analogous to intoxication, but much more violent. His breath became irregular, his heart palpitated violently, he lost sight of present objects, and a loud buzzing noise rung constantly in his ears. At this time his mental perception and power of association became wonderfully enhanced, and "thoughts that breathe and words that burn," came and went at com-

mand. When the paroxysm ceased, however, he fell into a state of great exhaustion, and lay motionless on the floor for *five* and *six* days together. Whilst the lecturer was in Paris he consulted his friends, Drs. Broussais, Magendie, and Edwards, and all of them agreed in pronouncing him mad. To convince him of the truth of their opinion, Rostan and Magendie took him to the Salpetrière, and showed him a lunatic patient who was highly accomplished, and thought justly on all subjects save one, which, when touched upon, rendered him outrageous. The Marquis saw all this, and yet was not convinced of his own insanity. He then gave slight sketches of several well-known improvisatoirs, both ancient and modern, learned as well as unlearned. Amongst the most renowned of these impromptu-poets, a Neapolitan Lazaroni, whose name we understood to be Luciato. These Lazaroni are great admirers of Ariosto, and are divided into two grand parties, the admirers of Rinaldo, and the admirers of Tancredi, and their improvisatoirs, of whom they have several, are employed in a constant poetical warfare on this subject. Luciato was entirely uneducated, and could neither read nor write, yet he was exceedingly intelligent, and had acquired a respectable fund of information. The Marquis and a friend, a physician, sought an interview with this Homer of the Lazaroni, and with *golden* arguments invoked the sybil of his poetic genius. The subject selected was—"*the distinction between a physician and an assassin.*" The Lazaroni replied in a string of correct Italian verses, animated with great wit and fancy. The point of satire upon which the whole turned was this:—"The assassin," says the poet, "first kills a man, and then takes away all that he has. The physician first takes every thing that a man has, and then kills him. *Voila la difference.*" In conclusion, the Marquis, in order to prove himself a true Arcadian, offered to improvise upon any two or three subjects the audience might choose. The mode of proceeding was the following. The auditory select a given number of rhymes which are to be included by the poet in his versification, thus preventing the possibility of the poet coming with a prepared subject. The subject is then chosen also by the auditory. The Marquis, without hesitation, recited the required number of couplets, upon the two following serious and comic subjects—*La Morte de Patroclo* and *La Tirania de l'Amore*, including in both pieces the *same* rhymes. There was much spirit in both productions, but the inspiration of which the Marquis spoke was not very discernible.

The Marquis de Moscati is a highly ingenious and talented man. He is an exile from his country, and has forfeited his estates in consequence of his attempts to liberate his native land from the grasp of foreign tyrants. He is consequently reduced, during his banishment in this country, to direct his talents to the gaining of a livelihood. He is deeply versed in Italian literature of all ages, and is about to deliver a course of lectures on his native language at Week's Rooms, Great Windmill Street, formerly the Hunterian School. We recommend such of our readers as have a penchant for the beautiful *lingua Toscana*, to patronise this amiable and high-minded nobleman in his laudable efforts.

THE LANCET.

London, Saturday, May 26th, 1832.

The last number of The Lancet had scarcely been sent to press when the melancholy intelligence reached London of the decease of Dr. Andrew Duncan, the late inestimable Professor of Materia Medica in the University of Edinburgh. The fall of so great a pillar of this ancient edifice is an event which may well awaken the attention, and excite the sorrow, of the medical profession.

It would, indeed, be a vain and purposeless task to recapitulate in these columns the many items which swelled the account of fame of the deceased Professor,—which gave him an honoured place among the children of science in every nation, and which will hand down his name to future times in association with the best and brightest ornaments of medicine. It would be vain for us, we repeat, to attempt the task. It baffles our ability as much as its fulfilment would exceed our limits; while there is not, we believe, that lisper in our profession to whom Dr. Duncan's attainments are not familiar, and by whom the nature of his many claims on professional gratitude is not fully appreciated.

The late Andrew Duncan was one of the rare examples which the history of medicine

supplies, of a man eminent in all the numerous and dissimilar departments, of which the abstract and almost ideal term "medicine" is constituted. An accurate anatomist and physiologist, he was the first to teach the rational method of dissecting the heart, and demonstrating the direction of its fibres. and he was amongst the foremost who directed attention to urethral and vesical malformations. A surgical pathologist, he established the existence of a new species of inflammation, erroneously regarded by previous and contemporary writers. A profound and original chemist, he discovered *cinchonine*, and thus lit up the lamp which enabled Pelletier and Caventou to detect and demonstrate the existence of quinine, and many other vegetable alkalies. In this capacity also he left us an additional monument of his skill in the analysis of mudar and detection of mudarine, the results of which are given to the profession in the last paper published before his decease. As a practical medical botanist he stood unrivalled; of his ability as a clinical teacher and practitioner our pages show splendid evidence. His fame as an author is based on the firm structure of twelve editions of his voluminous and invaluable Dispensatory. As a writer, he was distinguished for his perspicuity, his reasoning powers, and the modesty and amiable polish of his style. As a lecturer, his profound knowledge of every subject on which he treated, was equalled only by his zeal for the communication of that learning to his classes. The pupils' true and steady friend, he was their stern though merciful examiner; their willing and generous attendant in the hour of sickness; their kind, encouraging, and hospitable patron wherever merit deserved his peculiar favour; he was the defender of their privileges, and the active reformer of the wrongs by which they were aggrieved. Such is a scant epitome of Dr. Duncan's public merits, while in private life his diversified accomplishments his disciplined and excellent taste, and his innate benevolence of heart, made him all that an affectionate family should love, and that the more extensive circles of society should value and admire.

We speak these things of the departed Duncan, in the terms of exact though melancholy truth. His colleagues and old pupils intend, we understand, to place his bust in marble in the Library Hall of the University. We trust that no inscription will deface the pedestal. The memory of such a man requires no epitaph.

Next to the sorrow felt for the death of an illustrious public man, the most natural tendency of the human mind will lead to the contemplation of a fit successor in the public offices held by the deceased. In alluding to this subject, we must set out by stating that we know not, except by mere rumour, who are the candidates for the vacant Professorship. Neither have we the least personal interest in the kind of appointment that may be made. Anxious, however, for the preservation of the high character the Edinburgh University still maintains, despite the absurd and barbarous jurisdiction to which it is subjected, we willingly take up the theme, and proceed to inquire briefly who is the person most worthy to assume the gown of the late Professor.

> "Is there none left on whom the robe of fire
> Falls like the prophet's mantle?"—

The unanimous voice of all who possess the least knowledge of the present state of the Edinburgh University, must reply, that within the walls of that institution, there is an individual so eminently qualified to discharge the duties of that Professorship, that nothing short of the most perfect blindness, or the grossest jobbing, will countenance any other appointment, should the distinguished gentleman (we of course allude to Dr. Christison) think proper to

accept the Professorship, and we emphatically call upon him, for the credit of the University, not to shrink from a post to which he is so peculiarly fitted.

We are perfectly convinced that had the late Dr. Duncan been asked to appoint a successor to his chair—one who would maintain the rank to which he had raised it, who would keep pace with science, and who possessed all the collateral accomplishments requisite for this peculiar Professorship, Dr. Christison would have been the instant object of selection. It is absolutely impossible to produce an abler candidate. Dr. Christison's medico-legal pursuits necessarily constitute him a botanist, anatomist, and physiologist. His excellent and varied writings give him a conspicuous rank among the pathologists and practitioners of the present day. As a chemist he stands unrivalled, and we are well aware of his proficiency in every subordinate acquirement which such a situation demands. And then if we take fame into the calculation, where can be found another candidate, more extensively and favourably known to the professional world in every civilized country? He is the author of the best book but one which the Edinburgh University has produced, and though last, not least, he possesses physical qualifications for excellence as a lecturer of the most rare and valuable kind.

If then the venerable Baillies, &c., whose influence goes far to decide these appointments, retain a particle of honesty or national feeling, they will, without hesitation, *offer the office* to Dr. Christison. They should remember, that a rival University is daily shooting into vigour, and that it is only according to preponderance of talent that public favour will be awarded to one or the other of these institutions.

It has been stated to us, as an objection to Dr. Christison's appointment, that his services are so valuable as a lecturer on forensic medicine, that he must be retained in that position. The immaculate propriety of the Town Council could by no means sanction the holding of two professorships by the same individual—Pshaw! This is too thin a veil to hide the mammon of place and patronage. The professorships do not clash in the least degree. The pursuits of one, favour, instead of counteracting, the duties of the other. It would be rank injustice towards Dr. Christison and the public not to insist on his retaining both appointments.

We beg to call attention to the papers on the chemical pathology of cholera, inserted in this day's LANCET. It will be seen that Dr. Clanny offers no reply to our strictures. As to his claiming merit for the coincidence of his results with those of Dr. Thomson, we have only to remark, that Dr. O'Shaughnessy's observations on the experiments of the latter, save us the trouble of saying more, than that the process employed by Dr. Clanny for appreciating the amount of colouring matter (*not free carbon*) is the same as that employed by Dr. Thomson, and which Dr. O'Shaughnessy shows to be exposed to several sources of fallacy.

LONDON HOSPITAL.

COMPOUND FRACTURE OF THE SKULL, WITH DEPRESSION.

R. A., ætat. 59, was brought into the Accident Ward of this Hospital on the 26th of March, with compound fracture of the os frontis. While walking in the road he had been thrown down, and fell with his head against the edge of the curb-stone; he was stunned by the blow, but quickly recovered, and when brought into hospital immediately afterwards, was perfectly sensible. On examining him, there was found to be a contused wound, situate a little above the left superciliary ridge, being about four inches in length; and around this to some extent, a depression of bone was very perceptible. Notwithstanding this depression of bone, the patient did not manifest any symptoms of compression of

the brain. He was able to answer, distinctly, questions which were put to him; the breathing was free from stertor, and the pupils were obedient to light; the pulse about 62, and the motions of the body perfect; it was therefore not deemed necessary to elevate the depressed bone. He was bled to sixteen ounces, and cold lotion was applied to the head. He was ordered to take two grains of calomel every three hours.

28. Pulse 74, strong; tongue tolerably clean; pupils natural; complains of pain over the wound; he is perfectly sensible; has had very little sleep; bowels open twice.

29. Pulse 86, full; bowels have not been opened to-day.

30. Slept well; pain in the head less; pulse 78, full; tongue clean; bowels open; complains of his mouth being sore.

31. Free from pain; bowels open; sleeps well; has very little pain in the head.

April 1. Pulse 84, full, but soft; bowels open six times; passed a very restless night; complains of the calomel griping and purging him very much; mouth very sore.

2. Pulse 78, full, but soft; free from thirst; tongue clean; skin cool; has no pain in the head; in every respect doing well; the wound looks healthy. From this date the patient has continued doing well, and in all probability he will leave the hospital in a few days with the wound entirely closed.

FRACTURE OF THE CRANIUM WITH DEPRESSION.—DEATH.

—— Smith, ætat. 62, was brought into this Hospital about eleven in the morning of the 18th April, labouring under fracture, with depression of the skull. The persons who brought him hither stated, that while employed with four other men removing a dyer's vat, in passing it out of a door-way it slipped, and he falling forwards at the same time, his head was jammed between it and the door-post.

When admitted he was sensible; pupils dilated; pulse slow; respiration free and easy, without any stertor. On examination, a fracture was detected on the left temple, with a depression of a portion of the temporal and parietal bones, about the size of a crown piece; he was bled to 20 ounces, and immediately after the bleeding, he vomited the contents of his stomach.

About two hours after his admission, symptoms of compression of the brain came on; the breathing became stertorous; pulse slow and labouring; pupils dilated and immoveable; lies perfectly insensible, as though in a deep sleep; can be roused on shaking him, or speaking loudly to him, but instantly relapses into a state of insensibility; it was, therefore, determined to elevate the depressed portions of bone, and the trephine was accordingly used, by the use of which, and Key's saw, two insulated portions of bone were removed.

About six o'clock in the evening, he became extremely restless and violent, so much so as to require the restraint of the strait-waistcoat. At eight o'clock considerable reaction had taken place; he was bled to eighteen ounces, and fifteen grains of calomel were given him; he was ordered to have four grains every four hours. On being visited by Mr. Scott at nine o'clock, the calomel was increased to ten grains every four hours, and he was ordered to have twelve leeches applied to the forehead.

19. On visiting him this morning, found he had passed a very quiet night; bowels open twice; breathing free from stertor; pulse 80 and soft; skin cool; to continue the calomel as before directed.

At six o'clock in the evening, his pulse having increased in quickness, he was bled to twelve ounces, after which he gradually sunk until nine o'clock, when he died.

We were not present at the post-mortem examination, but learned that the head was examined about sixteen hours after death, when a fracture was found extending from the left side across both orbits, and extending to the spot immediately opposite to the injury; there was slight extravasation of blood on the surface of the brain, and the lateral ventricles were filled with a bloody serum.

AFFECTION OF THE MEDIAN NERVE AFTER AMPUTATION.

On the 21st April, Mr. Luke dissected out a portion (about one third of an inch) of the median nerve, from the stump of the arm of Elizabeth Burkitt, ætat. 19.

About three years and a half since, this poor girl went into St. Thomas's Hospital, with injury to the left-hand, occasioned by a fender having fallen across the back of it. After being there for several months, it was found necessary to have the hand removed, owing to the excessive suppuration which had taken place, and by which her general health became very much impaired. She states, the stump healed kindly, but shortly afterwards, she suffered excruciating pain in the stump, and for which amputation was performed by Mr. Langstaff above the elbow; the stump healed up, but shortly afterwards, violent pain, with convulsive twitchings of the stump came on, which prevented her getting any rest night or day. She applied at this hospital for relief, and desired the stump might be removed at the

shoulder joint, but which was not acceded to, and this operation proposed instead, to which she gladly consented.

Immediately on the division of the nerve, the pain and spasmodic twitchings of the stump ceased. She was placed in bed, and ordered to be kept perfectly quiet. The day following the operation, she complained of severe pain in the head, her skin hot and feverish, for which she was ordered the saline mixture three times a day, half a drachm of the extract of hyoscyamus at night, and four leeches to each temple.

23d. Passed a very restless night, owing to the severe pain in the head; bowels open, tongue clean, pulse 84, skin hot, complains of great intolerance of light. *V. S. ad* ʒx ; *rept. mistura.* After a short time, these febrile symptoms gave way, and she then suffered retention of urine, for which the catheter was introduced night and morning.

May 12th. The operation appears to have entirely succeeded, the wound having very nearly healed, and there being no return of pain; there is considerable numbness in the stump. She feels extremely grateful for the operation, being now enabled to enjoy a degree of comfort to which she has been an entire stranger for the last three years. She feels so much relieved by the operation, that, to use her own expression, she can hardly think she is her own self.

T. R., ætat. 7, was admitted into this hospital on the 9th instant, with laceration of the fingers of the left hand, caused by the machinery of a flock mill.

The dresser immediately amputated the first phalanges of three of the fingers, since which the little fellow has done very well.

COMPOUND FRACTURE OF THE TIBIA AND FIBULA.—AMPUTATION.

On the 19th of April, Mr. Andrews amputated the lower extremity of T. W., labourer, ætat. 48. This man was admitted into the hospital as far back as the 26th December last, with compound fracture of the tibia and fibula, having been run over by a cart wheel. Shortly after his admission it was feared, from the extensive injury, it would be impossible to save the limb, and he was advised to submit to the operation; but which he refused. The imminent danger of his situation was explained to him, but he resolutely and firmly adhered to his former determination. The fractured portions of bone were brought as accurately together as could be, and the limb laid in splints. Excessive suppuration followed; the man became hectic; had profuse night sweats, small quick pulse; cough, with purulent expectoration and great wasting away. The operation has been several times proposed to him, but he always refused submitting to it.

19th. On examining the limb to-day, it was found that no union of the bones had taken place, abscesses occupying nearly the whole of the leg, and the man extremely reduced by hectic, although he has been allowed a liberal diet, with wine and porter. Under these circumstances he was again informed there was no chance of saving his life unless the limb was removed; he at length consented, and was at once taken to the operating theatre, when Mr. Andrews commenced by making a circular incision about three inches above the knee, dividing the integuments; the muscles were next divided, and then dissected back; after which the bone was sawn through, very little blood was lost, and ligatures were applied.

The first night after the operation passed extremely well, he felt quite easy and comfortable, and had very little pain in the stump; his cough was not quite so troublesome as it had been; pulse 108, small and weak; skin cool; spirits good.

21st. Pulse 88, weak, spirits and appetite good, expectoration much diminished, tongue clean, bowels open, skin cool, feels quite comfortable.

23d. The stump was dressed to-day, looks remarkably well, adhesion having taken place throughout nearly the whole extent; there is a small quantity of healthy pus discharged; pulse 90, soft; bowels regular, appetite good, skin cool, tongue clean.

26th. The stump looks extremely well; discharge healthy, and in small quantity; most of the ligatures came away to-day; in every respect doing well.

28th. Continues improving; the last of the ligatures came away to-day.

May 12th. This case has continued improving. The stump looks remarkably well, being now nearly united. He was ordered to sit up daily; and from his progressive improvement there is every reason to suppose he will shortly be in a state to leave the house. Although this case has terminated favourably, it is greatly to be regretted that this man obstinately refused for so long a period to submit to a necessary, and as the case has proved, an ultimately inevitable, operation, as by so doing the cure has not only been protracted but his life endangered.

WESTMINSTER HOSPITAL.

GANGRENE OF THE LUNGS, VOMICÆ, AND PNEUMATOSIS THORACIS, WITHOUT COMMUNICATION WITH THE BRONCHIÆ.

April 27, 1832. William Hill, a smith, thirty years old, had enjoyed a fair share of health all his lifetime, but was never particularly hale. He was much addicted to drinking, and led a very irregular course of life. He in consequence became very susceptible of attacks of cold upon the lungs, which, however, were always quickly removed by rest, regimen, and medicine. In the winter-time, for the last few seasons, he had been annoyed with a pertinacious cough, which was short, frequent, and unattended with expectoration. The surface of his body did not show any cicatrices or other vestiges of syphilitic action. He was well formed, of good symmetry, moderately fine complexion, and well developed muscular system. About a fortnight ago, this man was affected with pain of the head and other febrile symptoms. He was obliged to desist from work immediately, and was attended by the parochial doctor, but without any benefit, and on admission to-day he presented the following picture. Countenance and the whole respiratory apparatus expressive of anxiety. Breathing accelerated; respiratory murmur enhanced on both sides of thorax; sense of constriction and pain in the chest; headach, nausea, and general debility; tongue coated with a brown fur; dry along its median line; red at its apex and edges; no appetite; bowels open; urine copious and high-coloured; pulse strong and hard; skin hot and dry. *A calomel and antimonial bolus to be swallowed directly; venesection to sixteen ounces, and the following medicine every four hours:—Take of ipecacuanha wine, three drachms; distilled water, eight ounces. Mix, and let an ounce of it be taken for a dose.*

29. The blood drawn is highly buffed and cupped. Last night he was seized with a shivering fit, which was followed by heat and sweating. There is a loud *râle sonore* on every aspect of the chest. He has an acute pain at the scrobiculus cordis; tongue moist and white; bowels open. *A blister to the pit of stomach; repeat medicines.*

Two p.m. Great excitement of respiration and sanguiferous system; has just recovered from a fit of rigors, and breathes 80 times per minute. Pulse quick, full, and hard; tongue covered with dark brown fur; skin hot and dry; pressure on abdomen produces great pain. He complains of acute pain under the floating ribs on both sides, which is much aggravated by the act of drinking. Râle sonore very loud. Venesection to twelve ounces.

30. Somewhat relieved by the loss of blood, which is again buffed and cupped, but the respiration is still as rapid as ever, attended with occasional gasping. Percussion *dull* on the right side of chest; râle sonore loud and strong, but modified by a *peculiar vibratory sensation*. The voice is feeble, and produces no pectoral reverberation. The patient always refers the pain to the epigastrium. *Cupping of the sides; mustard cataplasm to abdomen.*

May 1. The pulse has, for the last two days, been very irregular, varying every minute in force, frequency, and character. The anxiety of the patient's countenance is rather augmented than diminished. Dyspnœa intolerable; râle sonore has a *ringing* character. He expired at noon.

Necrotomy 48 Hours after Death.

The patient was not much emaciated, but his contracted visage even then commemorated the agony of the struggle he had undergone. Upon raising the sternum the pleuræ were discovered distended with air, and on opening these bags the lungs were seen compressed against the mediastinum and vertebral column. There was on both sides a large quantity of serum, in which were floating flocculi of lymph. A strong gangrenous stench emanated from the lungs, and on separating a portion of the external surface of the superior left lobe from the thoracic wall a large cavity was opened, of which the greater part was engraven in the substance of the lung. A portion of its parietes, however, also consisted in the costal pleura to which the lung was adherent. This sac contained a drachm of highly offensive sanies, and was itself gangrenous to a great extent. A small aperture communicated with the interpleural space, and was evidently the consequence of the sphacelation of the point of the pleura pulmonalis. The contents of this vomical sac had escaped between the pleuræ, but no direct or satisfactory communication could be ascertained to exist between the morbid cavity and the bronchial tubes. Two other solutions of continuity, similar to the one just described, were discovered communicating with smaller vomicæ. On making longitudinal incisions through the lungs, the substance or parenchyma was found to be in general pretty healthy, although, in consequence of the external pressure, a few air-bubbles only could be perceived in them. Several small abscesses, of the same character as the first were met with, dispersed through the substance of both lungs, distant from each other, and abruptly separated from the surrounding tissue by a dense sac. These

were filled with putrid pus, and had no outlet, either to bronchiæ or pleura. A small cluster of white pulpy tubercles, forming altogether a nidus not so large as a small hazel nut, appeared in the course of an incision through the apex of left lung. The whole of this little aggregate was enclosed in a thick membranous tissue, which probably formed the sac of the future vomica. On the right side there was a greater secretion of lymph amongst the serum than on the left, but without lung, though a due share of abscesses were present; no tubercular deposits were encountered. The largest vomica on the right side was in the upper part of the inferior lobe, and had an opening through the pleura. There was a remarkable healthiness in the texture of both lungs, even in those portions which were in immediate contact with the very walls of the vomicæ. These vomicæ were rendered apparently impervious to the air of the bronchiæ, by the coriaceous sacs which involved them. It is probable, however, that the air which distended the two pleuræ penetrated the texture of these sacs. The whole of the mucous membrane was highly injected, and afforded indications of long-standing inflammation. The projecting edges of membrane, seen at the axillæ of the bronchiæ, were universally thickened. The larynx was sound. In the other divisions of the body, no morbid traces were discernible.

ST. BARTHOLOMEW'S HOSPITAL.

CUT THROAT—HEMORRHAGE FROM THE LINGUAL ARTERY.—DEATH.

May 15. ——— was admitted into Powell's Ward about five days ago. He was a man-servant, and in a state of high excitement had seized a penknife, and inflicted a wound just above the os hyoides. This act was preceded, we believe, by an attempt to drown himself. Upon admission we did not see the case, but there was, we understand, a good deal of hemorrhage, which appeared to be chiefly venous; this however, was shortly put a stop to without any vessels being secured, as it was not known that any artery of consequence had been divided. The state of the man's mind soon became tranquil, he lay calmly in bed, and seemed likely to go on very well. Yesterday morning hemorrhage suddenly came on, which the sister arrested by cold applications. About eleven o'clock last night, *having returned by himself from the water-closet*, and got into bed, the attention of the nurse was suddenly called to him by a brief ejaculation. She found him with red blood streaming from his nose and mouth, and although Mr. M'Whinnie, the house surgeon, arrived in a minute or two, he found the patient lifeless, having lost about three quarts of blood. This unexpected termination of the case excited much interest in the *autopsy* which was instituted to-day, and the most rational way of accounting for death appeared to be the supposition that some vessel unknown to be wounded had been plugged up by a clot of coagulum, which was disturbed by the exertion of going to the water-closet. Upon examining the body, the excessive pallor of the skin, the empty state of the heart, and the vascular condition of the brain, described as present, under such circumstances, came under observation. There was an ounce and a half of fluid in the ventricles of the latter organ. Dissecting out the vessels around the wound, it was found that the *lingual* artery of the left side had been divided, and the extremity lay exposed in the gash which had been made. The other blood-vessels remained entire. It is proper to state, that the man had no instrument within his reach with which he could have effected a second cut.

ANOTHER CASE.—DEATH IN THREE WEEKS.

April 28th. A— B—, a strong, stout man, ætat. 56, was this morning brought to Coulson's Ward by some policemen. This was another case of cut throat. The account given by the policemen was, that about one hour previously the man, who was a cobbler, entered a barber's shop. There he was shaved; after which, taking up the razor in the barber's absence, he inflicted on his throat the present injury. His shirt, under-waistcoat, and upper clothes, are saturated with blood, but there is now very slight hemorrhage. His face is pallid; pulse scarcely perceptible. His eyes are closed, and great determination is imprinted on his countenance. Once he has opened his lips to exclaim, "I'm not a villain"—marking some conflict in his mind. Upon removing his garments the wound was found with some strappings on it, and ligatures applied on two or three small vessels. The dressings being removed, one very large gash was discovered, dividing the windpipe at the upper edge of the thyroid cartilage, and extending far laterally so as just to escape the great vessels. Besides this cut there were three smaller ones transversely, and one perpendicularly down the front of the larynx, but these were merely through the integuments. The wound was sponged and cleaned, and the edges approximated by raising the head and neck on pillows. Brandy was administered immediately on his admission, and con-

tinued afterwards, much escaping through the wound. The pulse soon acquired a more natural power. Head to be shaved. *Acetat. Morphiæ*, gr. ¼, statim.

29th. Has been very restless through the night; frequent delirium; persists in a wish to destroy himself, and even attempted to do so by hanging. Upon the administration of food by the stomach-pump he tries to push the tube into the larynx. Ordered *Vin. Antim.*, and apply *Emplastrum lyttæ amplum* to the nape of the neck.

30th. Outrageous during the night, but quiet to-day. The blister has been applied. Wound is sloughing and looks very formidable.

May 1st. Much the same. Continued thirst, to alleviate which cool spring water is applied to his lips; pulse moderate.

3rd. Extensive sloughing of the wound, causing a very copious and fetid discharge; mind appears more easy. The *morphia* is administered as need may be. A large bread poultice is to be applied over the whole wound.

6th. The poultice appears to have had a good effect on the wound, which looks somewhat cleaner and healthier.

From this time there is little use in giving a regular report. Suffice it to say, that from the state of excitement, the little nourishment which could be conveyed into his body brought him to a very low state, whilst the wound made no progress towards improvement. He subsisted on arrow-root, but was gradually sinking. This state of things continued until the 19th, on the night of which, after being in vain stimulated with doses of brandy, he died quietly.

21. The *post-mortem* inspection was obtained to-day. The body was much emaciated, the fat being absorbed, except here and there, where an isolated lump remained on the legs and arms. Wound dirty, ragged, ununited in any part, and forming the source of a muddy-looking discharge which burrowed down, beside the larynx, towards the cavity of the chest. The thyroid cartilage was laid bare by the sloughing process. Inside of larynx and trachea paler than natural; lungs and pleura healthy, as also the abdominal viscera. The examination of the brain and its investments exhibited nothing peculiar, except a slight increase of vascularity in the latter, and a more soft state than is natural to the former. The fluid in the ventricles was about equal to the usual quantity.

The history of this patient is not known further than that he is said to have been incited to suicide by taunts raised against him by some companions.

PROPOSAL TO FRANK ALL MEDICAL PERIODICALS.

To the Editor of The Lancet.

Sir,—I observed in the title-page of the *Cholera Gazette*, "FRANKED to any part of the kingdom." Now it is not my wish or intention to quarrel with this piece of liberality, deeming it a very proper facility for circulating a publication which certainly *ought* to possess great value; but, Sir, as there are several weekly and monthly periodicals, wholly devoted to the purposes of disseminating equally, not to say more, useful medical information, I should very much like to know, why this boon of FRANKED might not be extended to them also? As the desire of doing good solely, no doubt, influenced this free transit of the Gazette, would not an extension of this privilege be eminently beneficial to science, and consequently to health? Pray turn your attention to this point; and, as you are so capable of taking an enlarged and enlightened view of subjects in general, see if you cannot so unfold my meagre suggestion, as to show at once its justice and reasonableness in such a manner as to secure, if not its adoption, at least the hearty exertions of the profession for ultimately securing it. Being a metropolitan, no advantage would accrue to myself, but my "country cousins" would feel it by the earlier receipt of their mental food. I am, Sir,

An Invalid Subscriber to The Lancet.

TO CORRESPONDENTS.

Communications have reached us from Mr. E. F. Williams—A Surgical Reformer—U. X.—Mr. W. Haylett—Mr. Bedingfield—Philo-Lancet—Dr. Harrison—S. P.—A Bartholomew Student—Mr. P. Summers, Mr. Waters, and other gentlemen.

The notice sent by our friend, in the neighbourhood of Cavendish Square, would be charged at the Stamp Office as an advertisement.

Dr. Ayre's paper on the treatment of cholera reached us too late for insertion, but it shall appear in our next Number.

Want of space has compelled us to postpone the publication of several communications till next week. Amongst others, interesting cases forwarded by Dr. Wetherall of Liverpool.

The acknowledgment on the part of T— is not less creditable to his veracity than to his manly sincerity.

Inquirer. We understand that several of the gentlemen *did* signify their disapprobation of the toast. No one can doubt what they *ought* to have done.

The complaints of *H. W.—A Dresser*— and others, shall be kept in view. If their own remonstrances fail of effect, we will try what can be accomplished by a little public castigation.

Mr. R. D——. The paper *was* received. It is under consideration.